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PSYCHOANALYSIS — MYTH OR SCIENCE?

by

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In this paper an attempt is made to look at Freud's contribution from the point of view of its scientific validity. A factual survey is made of the results of psychoanalytic psychotherapy, of the kinds of facts and arguments used to support the psychoanalytic doctrine and of the experiments carried out to test it. The conclusion arrived at is that psychoanalysis and the theories associated with it is not a science, but a myth; adherence to it is based on emotion and prejudice rather than on fact and reason.

I. Introduction

Psychoanalysis presents a rather curious dilemma to those who would evaluate it. In psychiatry it has become the leading school to such an extent that in some countries, particularly the United States, it is almost impossible to obtain a leading post, either in academic life or private practice, without having undergone a training in analysis and thus having been exposed to a most efficient form of "brain washing". Similarly, among novelists, film makers, journalists, teachers, philosophers, and even among the general public, psychoanalysis is almost the only type of psychology at all well known; indeed, to most people, psychoanalysis *is* psychology. Even cartoonists have joined in this chorus of agreement, to the extent that "the psychoanalysis joke" has become as much of a standby in *Punch* as in *The New Yorker*. In a very short time Freud's original heresies have thus become widely accepted, and even more widely acclaimed — a fact which seems to go counter to Freud's own deduction from his theories that there would be a particularly strong and virulent *resistance* to the acceptance of psychoanalysis, as compared with other new beliefs. In actual fact, psychoanalysis was accepted much more readily, much more widely, and much more uncritically than almost any other set of comparable, revolutionary and new ideas. If Freud's prediction

does indeed follow from his theory, then the facts seem to have disproved this prediction as well as so many others.

In all this chorus of jubilation, however, there is still a hard core of unbelievers; a group of people to whom the whole story of psychoanalysis is little but a repetition of the famous fairy-tale about the Emperor's new clothes. And it is curious to note that these dissenters tend to be found mostly among those who have been trained in scientific method and who have adopted psychology as their profession. There are very few experimental psychologists or leading psychological theoreticians who accept the Freudian doctrine, and the majority tend to regard it as so much beyond the pale that they do not even consider it necessary to discuss and argue its pretensions. We thus have the curious position that psychoanalysis is widely accepted among lay people and others untrained in psychology, ignorant of experimental methods and incapable of evaluating empirical evidence. On the other hand, we have a widespread rejection of psychoanalytic claims by those knowledgeable in psychology, experienced in experimental methodology and well able to evaluate empirical findings. The most obvious hypothesis suggested by this state of affairs would seem to be that psychoanalysis is a myth; a set of semi-religious beliefs disseminated by a group of people who should be regarded as prophets rather than scientists. It will be the purpose of this article to investigate to what extent this hypothesis may contain seeds of truth, and to what extent it may be a mischievous caricature of the state of affairs as it exists at present.

First of all, however, we must deal with a type of argument which is sometimes presented by supporters of the psychoanalytic movement. What is said is something like this. Psychology by its very nature cannot be a *Naturwissenschaft*, i.e. a natural science like physics or physiology, but it must be a *Geisteswissenschaft*, i.e. a kind of intuitive, humanistic discipline; that psychology cannot *explain* behaviour in terms of general laws, but can only *understand* it in terms of each individual's own intuitions. This is a line taken, among others, by Husserl and other German philosophers whose *a priori*, *ex cathedra obiter dicta* have attracted far more attention than their lack of factual information of modern psychology and its ways of working would seem to warrant. Fortunately, however, it will not be necessary here to deal with the erroneous arguments of what Windelband has christened the *ideographic* school, as compared with the *nomothetic* school; we need only look at Freud's own claims, and those of his closest followers, to realize

that he himself would not have wished to hide behind the skirts of this particularly unattractive mother figure. He believed, and stated unambiguously on many occasions, that psychoanalysis was a scientific discipline like any other, that its laws had the same claims to universality as do those of physics; that its predictions were scientific predictions which could be tested empirically, and that the whole outlook and tenet of psychoanalysis was deterministic. Those who now wish to escape from the consequences of the empirical testing of psychoanalytic doctrines by claiming that psychoanalysis should be judged on other terms, are presenting us with an argument which would not have appealed to Freud at all, and I will not attempt here to deal with these evasions of the central issue. The only interest the Freudian doctrines have lies in their factual content, and in the conclusions that flow from this content in the way of psychotherapeutic treatment. It is always possible to defend a religious belief on non-empirical grounds: "I know that my Redeemer liveth" is not intended to be a scientific statement equivalent to "I know that the neutrino exists". If we now make such a statement as "the Oedipus complex is universal and has certain definite behavioural consequences" we can take this as equivalent either to the religious statement, to be chanted in unison at psychoanalytic conferences but having no relevance to factual matters, or we can regard it as a statement of the scientific kind to be evaluated in empirical terms. I would like to suggest that if psychoanalytic statements are of the former type, they are of no interest whatsoever, except to the student of religious beliefs. If they are of the latter type, then the interest is in direct proportion to the amount of evidence which can be brought to support them. In other words, psychoanalysis is a science, subject to the usual dictates of scientific argument and scientific evidence, or it is nothing.

II. Effects of Psychotherapy

Psychoanalysis was originally introduced as a method of treatment of neurotic disorders, and as a theory to explain the causation of disorders. The theory has undergone many subtle changes, and I shall assume it to be too well known to require restatement except in the very briefest outline. To the psychoanalyst neurotic symptoms are merely the observable signs of underlying complexes, repressed well into the unconscious but too strong to remain completely suppressed. These complexes date back to childhood years and are associated with

the Oedipus complex which is their *fons et origo*. Treatment consists in *uncovering* the original infantile experience which laid the basis for the later neurosis.

This type of treatment has now been going on for some sixty years, and many thousands of psychiatrists and psychoanalysts have been practising it in practically all the civilized countries of the world. One would imagine that after all this time some definite knowledge would have accumulated about the effectiveness of psychotherapy as so practised. This, it is interesting to report, is not so. Psychoanalysts have always been eager to hide their light under a bushel as far as evidence of the success or otherwise of their treatment is concerned. This contrasts rather sharply with the impression, given wittingly or unwittingly by psychoanalysts, that their method is the only one which gives positive and lasting results in this field. What psychoanalysts have usually done has been to publish individual cases, almost invariably cases in which the patient got better, and to argue from these illustrative examples to the general case. The argument may be formally stated in a way that exposes it as one of the classical examples of the *post hoc ergo propter hoc* fallacy. The fact that a patient, John Doe, who is suffering from a phobia, gets better four years after psychoanalytic treatment has been initiated, is not proof that John Doe has got better *because* of such psychoanalytic treatment, and to reason thus even by implication, is so obviously absurd that I will not waste space by arguing the case. There is no method of treatment, from prayer to giving neurotics cold baths, and from hypnosis to extracting their teeth in order to eliminate septic foci, which has not given rise to similar claims to those of psychoanalysis, and which has not published clamorous and lengthy accounts of "cures" so accomplished. Clearly the assessment of therapeutic claims in this field is complex and difficult and requires a certain degree of sophistication.

The most obvious difficulty that arises is the problem of what is sometimes called *spontaneous remission*. It is well known that neurotic disorders often clear up without any formal treatment of any kind; indeed this is true of the majority of cases. They also clear up after types of treatment which are completely non-specific and which, according to the psychoanalysts, should have no effect at all. A particularly good example is the famous study of Denker in which he studied five hundred severe neurotics who had complete disability pensions because of their neuroses. Not only did these five hundred fail to receive any kind of psychoanalytic treatment; they were also,

because of their pensions, highly motivated to retain their illness. Nevertheless, some two out of three completely recovered within two years, having had no other treatment than the usual pink pills and pep talks of their G.P.s. After five years the percentage of recoveries rose to some 90 per cent. There are many other studies giving rise to similar conclusions, to wit, that neurotic disorders are generally of a self-terminating kind and, however severe, are not likely to last for more than two or three years even when left untreated, or when treated by people with no training in psychiatry or psychoanalysis.¹

To prove its efficacy, psychoanalysis would clearly have to do better than this. If people treated by psychoanalysis did not recover more quickly or in greater numbers than when left untreated, then clearly the claims of psychoanalysis, as far as its curative powers are concerned, would have to be rejected. Actually one might anticipate a positive showing for psychoanalysis even though the method was not in fact efficacious. The reasons for this are as follows. Psychoanalysts, by and large, only treat the better-off and more intelligent types of patient, and furthermore they tend to select their patients very stringently in terms of their likelihood to benefit from treatment. On these grounds their patients should have a better recovery rate than the more unselected groups on which the spontaneous recovery base line was established. In actual fact the data suggest very strongly that, if anything, patients treated by psychoanalysis *take longer to recover and recover to a lesser extent* than do patients left untreated. This conclusion is arrived at by averaging the claims made by various psychoanalysts and psychoanalytic institutions with respect to their patients. These claims are taken at face value, although there is the ever-present danger that each analyst would be prejudiced in favour of his own successes, thus giving a more optimistic view than would be warranted had an independent examination been made of the patients.

Such an actuarial comparison is, of course, defective from many points of view. It is difficult to be certain that the persons in the various groups are in fact suffering from equally serious disorders; and it is difficult to be sure that the criteria of "cure" and "recovery" used by different people are in fact identical. Much could be said in relation to both these points, but however much we might be willing to favour the psychoanalytic side, and however much our assumptions might strain probabilities, yet on no account can the figures be interpreted to give any support whatsoever for psychoanalytic claims. This verdict is borne out by several studies, much better controlled experimentally,

where patients have been divided into various groups, submitted respectively to treatments of various kinds or no treatment at all. The results of these studies bear out the findings that psychoanalysis has no apparent effect as compared with other treatments or no treatment at all; again, therefore, psychoanalytic treatment receives no support from the outcome of the experiment.

One might have thought that, with respect to children, psychoanalysis might be more positively placed, as these might be considered to be more impressionable and more easily cured. Here also, however, an extensive review of the literature shows a picture almost identical in every detail with that found in adults. There is no evidence that psychoanalysis of children produces any kind of effect on the neurotic symptoms of these children.

In 1952 I published a short paper listing the evidence and describing what I thought was the only possible conclusion to which it could lead, to wit, that the null hypothesis had not been disproved, i.e. that psychoanalysts had failed to show that their methods produced any ameliorating effects on people suffering from neurotic disorders. This brief, factual, and innocuous paper produced a whole shower of replies, critiques, refutations, arguments, and discussions; it did not, however, produce a single mention of a single experiment or clinical trial which had demonstrated a positive effect for psychoanalytic treatment. Indeed, in recent years the more official and better-informed psychoanalysts have become rather more chary of making any claims of therapeutic effectiveness for psychoanalysis. Glover, to take but one example, has explicitly rejected such claims in his latest book; the Chairman of the Fact Finding Committee of the American Psychoanalytic Association has explicitly stated that his Association had no positive evidence on the point, and did not make any kind of claim of therapeutic usefulness; Schmiedeberg and many other practising analysts have come to similar conclusions in print. It has been left to the large herd of faithful believers, who have no direct knowledge of psychoanalytic practices and are ignorant of the very existence of a large experimental literature, to continue to make claims which are not, in any way, supported by the evidence.

Why is it, the reader may ask, that in spite of its apparent uselessness, psychotherapy is so widely praised by people who have undergone it, and who claim they have been cured by it? The answer I think lies in a famous experiment, reported by the American psychologist, B. F. Skinner. He left a group of pigeons alone in their cage for

twelve hours but arranged for an automatic hopper to throw out a few grains of corn at intervals to the hungry animals. When Skinner returned in the morning, he found that the animals were behaving in a very odd manner. Some were jumping up and down on one leg, some were pirouetting about with one wing in the air; others again were stretching the neck as high as it would go. What had happened? The animals, in the course of their explorations, had happened to make that particular movement when the hopper had released some corn. The pigeon, not being a slouch at the *post hoc ergo propter hoc* argument, imagined that the movement preceding the corn had, in fact, produced the corn, and immediately began to repeat the same movement again and again. When finally another reward came tumbling out of the hopper, the pigeon became more firmly convinced of the causal consequences, so throughout the twelve hours the pigeon performed the movement and the hopper, at irregular intervals, dispensed the corn. To leave out the anthropomorphic terminology, and to put it in slightly more respectable language, we may say that the pigeon became conditioned to make a particular response in order to receive a particular reward. There is nothing mysterious about the experiment, which Skinner entitled "A Study in the Growth of Superstition", and we can directly relate it to the growth of the belief in the efficacy of psychoanalytic treatment, both among patients and among psychoanalysts themselves.

Neurotics get better regardless of treatment; this improvement constitutes the reinforcement, and is equivalent to the corn received by the pigeon. The actions of the psychotherapist are as irrelevant as is the behaviour of the pigeon in the experimental situation. Neither is instrumental in producing the reinforcement, but both become connected with it through processes of conditioning; thus a superstition is created, both in the pigeon and in the patient, linking the one with the other. Much the same is true of the therapist himself; for him too, the reinforcement is the improvement reported by the patient. This is independent of his actions, but because it follows them in time, the conditioned response is established. There is nothing in the published evidence to contradict this hypothesis, and much to support it.

III. The Data of Psychoanalysis

It has often been said that psychoanalysis is more than a curative technique, and that a failure to prove the efficacy of psychotherapy

would not necessarily invalidate the truth of the psychoanalytic doctrine in other respects. (Conversely, it might be said that even if psychoanalysis were found to be a successful method of therapy, this would not necessarily prove the truth of the psychoanalytic doctrine.) Up to a point this may be true, but I think it should be accepted only with grave reservations. In the first place, the whole doctrine of psychoanalysis was based on information obtained during the treatment of neurotic patients and in the course of trying to effect an amelioration of their symptoms. To admit that the primary purpose of psychoanalysis had resulted in complete failure, but that nevertheless the doctrine was correct and scientifically valuable, seems, on the face of it, an unlikely contingency ("By their fruits shall ye know them!"). But this, of course, is not all. If the theory is correct, then the method of treatment would seem to develop from the theory, and what is more, it should work in practice. Conversely, if the theory of psychoanalysis is correct, then *spontaneous remission* and the various non-analytic methods of treatment should not be effective and should leave the individual, if anything, worse off rather than better. Thus we have a quite specific deduction from the hypothesis which the facts disprove very thoroughly indeed; I shall come back to this point a little later on. While it thus remains a theoretical possibility that parts, at least, of psychoanalysis might conceivably be correct, although its therapeutic methods were shown to be useless, nevertheless we would require very strong evidence indeed, before accepting such a conclusion. A great deal of experimental work has of course been done in attempts to verify or disprove parts of the psychoanalytic structure. This is not the place to review this very large body of work; it must suffice to say that, on the whole, it has been very detrimental to the psychoanalytic claims. In saying this I must make one important distinction. Most laymen completely misunderstand the Freudian doctrine, and, therefore, mistake as confirmatory evidence, facts which in reality are quite neutral. Freud used certain well-known facts in a rather peculiar manner; the facts themselves may be true, but their verification does not imply that his use of these facts was correct. As an example of this, let me take the concept of *symbolism*.

The facts of the matter are clearly consistent with the notion that we frequently use symbols in our discourse, in our writings, and possibly also in our dreams. These facts have been known for thousands of years; the reader may like to recall the biblical dream of The Seven Lean Kine and the Seven Fat Kine! Modern apologists of the psycho-

analytic movement sometimes write as if Freud had discovered symbolism — as well as sex and a great number of other important factors! His actual contribution, however, has been quite different. He has suggested a possible mechanism and reason for the use of symbols, and he has suggested ways of deciphering the symbolic language of the dream. I do not know of any evidence to indicate that these contributions have a factual basis, and I know many reasons why they should be considered highly unlikely.

Let us take only one or two considerations into account. In the first place, one and the same dream is often interpreted along entirely different lines by different analysts; frequently these accounts are contradictory. It would seem, therefore, that if any one account is “correct”, all the others must be false. We are not, however, given any means of deciding which is the “correct” account, nor is the possibility ruled out that all of these accounts are in fact erroneous and have no reference to reality. Analysts often suggest that the proof of the correctness of the interpretation can be found either in the fact that the patient accepts the interpretation, or else in the fact that the patient gets better after the interpretation has been made. Arguments of this kind are too illogical to deserve an extended reply; a patient’s “acceptance” of an analyst’s interpretation can hardly be regarded as scientific evidence. And as we have shown previously, the patients are likely to get better anyway, dream interpretation or no dream interpretation, and consequently the improvement is irrelevant to the truth or falsity of the theory. It must be admitted that in isolated and highly selected cases, a good case can sometimes be made out in favour of the Freudian notions. Thus consider the following example. A young girl dreams that a young man is trying to mount a rather frisky horse. He almost succeeds on two occasions and finally achieves success on the third. The analyst succeeds in elucidating the facts: (1) that the young man in the dream is the patient’s fiancé, and (2) that the patient’s nickname is “Cheval”. His interpretation to her is that she wishes to have intercourse with her fiancé, and she volunteers the information that on two occasions she and her fiancé went so far in their love-making that she only just succeeded in extricating herself. So far so good; here we seem to have an excellent example of Freudian symbolism at work, together with his notion of “wish fulfillment”. But remember that according to Freud’s theory, the reason for the use of symbols was simply that the matter dreamed about was too painful or too intolerable for the mind of the dreamer to be accepted

without disguise. Is it really acceptable to be asked to believe that a young girl who went as far as this in her love-making found the notion of intercourse so painful to contemplate that it had to be disguised in symbolic form? Far from supporting the Freudian position, therefore, this particular example would seem to demonstrate that while it is true that symbols which have been known for thousands of years do indeed occur in dreams, yet the particular Freudian contribution which explains the occurrence of these symbols, does not fit the facts at all. Thus data which superficially may seem to support the Freudian view, can often be found, on closer inspection, to contradict it significantly. It is the admixture of true and long-known facts which makes creditable to the unwary reader, the peculiar and unwarranted use made by Freud of these facts; it is this feature of his theory which has led one famous psychologist to say of it: "What is new in it is not true, and what is true in it is not new."

It may be said altogether that for Freud there was a distinct failure to comprehend a distinction between a fact and the interpretation of that fact. This failure is rendered less obvious than it would otherwise be by Freud's excellent command of language and by his skill in presenting his case to its best advantage. But woe betide the reader who tries to separate the facts from the interpretations, in order to discover whether or not the former can in truth be said to give rise in any unequivocal manner to the latter! He will find his task made almost impossible by the skilful way in which Freud has hidden and glossed over important facts, and the brilliant way in which he has highlighted his interpretive account of what may, should, or ought to have happened, but which, as far as one can discover, probably never did happen. As a supreme example of this, the reader is urged to go back to Freud's original writings and reread his "Analysis of a Phobia in a Five-year-old Boy" — the famous case of little Hans. This has achieved considerable historical importance and has been universally praised by psychoanalysts as the inauguration of all child analyses. Let us have a look at little Hans, who developed a fear of horses after having seen a horse, which was pulling a bus along the street, fall down in front of his eyes. It is noteworthy that Freud only had one short interview with little Hans; all the rest of the material was provided by the father of little Hans, who, we are told, was an ardent follower of Freud. The father, as will be seen by anyone reading through the account, is constantly telling little Hans what he wants him to say, and usually continues until little Hans (who after

all was only five years old) gave some kind of consent. When even this produced no results, the father had no hesitation in saying that Hans really meant exactly the opposite of what he actually said, then treating this, in itself, as an established fact. Freud seems to have realized this to some extent and says: "It is true that during the analysis Hans had to be told many things which he could not say himself, that he had to be presented with thoughts which he had so far shown no signs of possessing and that his attention had to be turned in the direction from which his father was expecting something to come. This detracts from the evidential value of the analysis but the procedure is the same in every case. For a psychoanalysis is not an impartial scientific investigation but a therapeutic measure." Freud, himself, followed exactly the same procedure as the father because in his interview with the boy he told him "that he was afraid of his father because he himself nourished jealous and hostile wishes against him". The boy, his introspections, his sayings and his thoughts, are never really in the picture; what we always get is what either his father or Freud told him he should think or feel on the basis of their particular hypothesis. And whether the child could finally be made to agree or not, the result was always interpreted as being a vindication of the theory. No one who has a scientist's almost instinctive veneration for facts can regard this psychoanalytic classic as anything but a straightforward attempt to fit the child's testimony into the Procrustean bed of a cut and dried theory, previously determined upon; it is difficult to imagine anything little Hans could have said or done that could not in this manner have been transfused into support of the theory. Even so, however, there are glaring cases of inconsistency in the account; thus little Hans was afraid of the "black things on the horses' mouths and the things in front of their eyes"; Freud claimed that this fear was based on moustaches and eyeglasses and had been "directly transposed from his father onto the horses". In actual fact the child was thinking of the muzzle and the blinkers which had been worn by the horse that fell. Again Freud interpreted the agoraphobic element of Hans's neurosis "as a means of allowing him to stay at home with his beloved mother". Nevertheless, both the horse phobia and the general agoraphobia were present even when little Hans went out with his mother!

A very detailed examination of this case has been made recently by S. Rachman and J. Wolpe in a paper published in the *Journal of Nervous and Mental Diseases*, (1960, 130, 135-48) and the reader who

wishes to form an independent and unbiased idea of the reliability and validity of psychoanalytic investigations is urged to read both the original case and this review of it. He will find that the case of little Hans is very similar to all other cases published by Freudian writers, in supporting a gigantic pyramid of speculation on a small pebble of fact.

IV. The Behaviourist Account of Neuroses²

Conant, of Harvard, has pointed out that no scientific theory has ever been killed by the criticism directed at its inadequacies; what is required is an alternative and clearly superior theory. Such a theory, in my view, is at the moment in the process of being formulated by a number of American and British writers; its theoretical background lies in Pavlovian conditioning and modern learning theory, while its practical application has been labelled "behaviour therapy", to indicate its relationship to the tenets of behaviourism. What is maintained by this theory may be put very briefly thus. Neurotic symptoms are maladaptive actions and/or emotions which have become conditioned to certain types of stimuli. They can be removed by an appropriate process of extinction or counter-conditioning. There is no disease underlying these symptoms, and there are no complexes which produce new symptoms should the old ones be extinguished. All that we are dealing with in a neurosis is, in fact, the symptom or set of symptoms; once these are eliminated, the neurosis, as such, has vanished.

A simple illustration may make clear the meaning of some of these terms. Consider another infant, this time little Albert, an eleven-month-old boy who was being studied by Watson, the originator of behaviourism. Watson had been impressed by Pavlov's demonstration in which a dog becomes conditioned to salivate to the sound of a bell by being given food a number of times just after the bell has been rung. After some twenty pairings of bell and food, the bell alone (the conditioned stimulus) now produces salivation where previous to the pairing with the food, it had failed to do so. Watson made use of this paradigm in conditioning a phobia for rats in little Albert, who used to be very fond of these animals. Watson simply stood behind the infant with a hammer and an iron bar, and whenever little Albert reached for the rat (the conditioned stimulus) Watson would bang the iron bar with the hammer, thus creating a loud noise which frightened

little Albert. After a few repetitions, little Albert, as had been predicted, became afraid of white rats, and developed a phobia for them; indeed, as had also been predicted, this phobia generalized to other furry animals, such as rabbits. We thus have the experimental production of a phobia through the use of a mechanism, well understood and widely studied in the experimental laboratory, both in animals and men. There is no talk here of hypothetical Oedipus complexes, unconscious ideas, super egos, and ids, and all the rest of the psychoanalytic hagiology.

Having established a phobia, can we also cure it by applying the techniques of the conditioning laboratory? The answer is in the affirmative. We have conditioned the infant to respond with fear to the rat; we must now condition the infant to respond with a positive emotion instead. This presents one difficulty; the infant is so frightened by the sight of the rat that he will not be in a fit state to form the conditioned response to the rat opposite in sign to that already established. This problem, fortunately, is not insuperable. The fear of the rat is in part a function of its distance from the infant; remove the rat to the farthest corner of the room and give the hungry infant a piece of chocolate (the unconditioned stimulus) and the infant will munch the chocolate whilst cautiously eying the rat in the far corner. Repeat these processes a few times, bringing the rat closer on each occasion and finally the infant will be munching his chocolate whilst playing with the rat. The phobia has been cured never to return.

It is noteworthy that this simple, straightforward hypothesis explains equally well all the facts in the story of little Hans. The fear of horses is accounted for in terms of the traumatic instance of the collapsing animal in front of the bus. (Indeed, the child had been sensitized by two prior experiences with horses.) This conditioned fear of horses, and the open space in which the accident took place, requires none of the mumbo jumbo with which Freud surrounds a perfectly simple and straightforward happening which can be duplicated any day in the laboratory. Indeed Hans, himself, emphatically supports this view. This is what he says: "No. I only got it [the phobia] then. When the horse and the bus fell down, it gave me such a fright, really! That was when I got the nonsense." And the father says: "All of this was confirmed by my wife, as well as the fact that the anxiety broke out immediately afterwards." This view of Hans's phobia is strongly supported by Rachman and Wolpe in the paper already alluded to, and they also advance a plausible view about the decon-

ditioning of little Hans's phobia. We can say, therefore, that simple and straightforward as the behaviouristic account may be, it nevertheless accounts for all the relevant facts in little Hans's sad history, and it does so without requiring a vast amount of speculative elaboration.

If such a theory is indeed, in principle, correct, then we should expect it to furnish us with methods of treatment considerably superior to those advocated by the psychoanalysts. This appears to be the case. J. Wolpe has developed a number of methods for treating neurotic disorders, all of which are based on modern learning theory and the hypothesis that neurotic symptoms are nothing but conditioned maladaptive responses of one kind or another. In his recently published book, "Psychotherapy by Reciprocal Inhibition", he has published statistical data comparing the degree of success of this type of treatment with the published figures of psychoanalytic treatment, showing that behaviour therapy is not only very much shorter than psychoanalysis, but is also very much more successful; with an average of less than thirty visits, he reports successes in some ninety per cent of all cases. Not too much should be made, of course, of statistics of this type, because of the well-known difficulties attending all such comparisons. Nevertheless, the experience of others who have used similar techniques bears out Wolpe's contention that here we have at long last a theory and a method which do enable us, which psychoanalysis never did, to come to grips with the widespread neurotic fears and anxieties which are so characteristic of our time, to understand them and to cure them. Psychoanalysis has survived for so long, in spite of its continued failure to provide a successful method of cure, because *natura abhorret vacuum*. As long as no alternative theory was available which could account for the facts of neurotic disorders, and which could suggest new and successful methods of treating these disorders, so long was psychoanalysis in a safe and impregnable position. Its scientific and philosophical pretensions have long since been stripped away, and it has survived largely through inertia and through the large body of vested interests which have grown up in its wake. It is unlikely that the Emperor's new clothes will be admired for very much longer.

NOTES

- ¹ It is, of course, impossible in a short paper like this to document one's statements sufficiently to carry conviction. The reader who is interested in a thorough review of the facts and in a detailed list of references, will find these in the *Handbook of Abnormal Psychology* (Pitman, 1960) which I have edited. Of particular relevance is the chapter on "The Effects of Psychotherapy".
- ² Space does not permit to deal adequately with the behaviourist's interpretation of neurotic disorders, and the description of behaviour therapy. The reader who is interested in more extensive documentation, may be directed to the present writer's *Behaviour Therapy and the Neuroses* (Pergamon Press, 1960) which contains a very full account, both of the theory of behaviour therapy, and also of large numbers of empirical studies using the concepts and methods of modern learning theory for the purpose of treatment of various neurotic symptoms.

CAN PSYCHOANALYSIS BE REFUTED?

by

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This paper examines the challenge that psychoanalytic theory cannot be refuted. It does so by considering the theory in its orthodox Freudian form, and in the main branches into which it can be divided — the theory of Instincts, of Development, of Psychic Structure, of Mental Economics or Defence, and of Symptom Formation. The essential character of the generalizations and concepts of these branches will just be indicated; and we shall ask of each branch whether it is possible to refute it. A considerable amount of scientific enquiry has been done into the concepts and generalizations of psychoanalysis. Relevant examples of these enquiries will be noted; and the question asked whether these scientific studies have in fact done anything to refute or support the various branches of psychoanalytic theory. The general upshot will be that the challenge is both important and a mistake.

I. Introduction

This question opens up a very large problem, and I shall only offer some thoughts on it in an abrupt and disjointed way. Because it is a large problem, I shall cut it down to more manageable size by restricting myself to psychoanalytic theory of the classical or orthodox Freudian sort. What we discover about this brand of psychoanalytic theory may not be without relevance to other brands, ancient, modern and deviant.

Is it logically possible to refute this theory? The problem is well known. Analysts have been challenged to show that it is refutable,¹ and they have been slow to meet the challenge. Its importance lies in the fact that, if the theory cannot be refuted in principle, we will have good reason for saying that is not a “scientific” theory at all, but something more akin to “a myth” (as Popper suggests), or just a “wonderful representation” of the facts (as Wittgenstein apparently took it to be²).

II. The Parts of Psychoanalytic Theory

As soon as we take up the challenge, we notice one thing about the theory at once. It is not a unified theory — it does not contain certain fundamental, or primitive, concepts which appear in certain basic postulates, and from which the rest of the theory is developed. Hence it is not possible to refute “the theory” by testing directly any of its lowest level generalizations; nor is it possible to refute it by deducing an empirical consequence from any generalization, and indirectly testing it by directly testing the deduced consequence. But this limitation of psychoanalytic theory does not appear to matter. The fact that the theory is not unified, and hence not refutable as such — as a whole — does not mean that the constituent parts of the theory are not refutable. Psychoanalysis may be in the stage that our knowledge of the physical world was before the unification of Physics got under way last century. So it is misleading to frame the challenge in the one big question: can psychoanalytic theory be refuted? We must reformulate it and ask: can each of the different parts of psychoanalytic theory be refuted?

One of the traditional ways of dividing up the theory is as follows:

1. The theory of Instincts, or Dynamics;
2. The theory of Development;
3. The theory of Psychic Structure;
4. The theory of Mental Economics, or Defence;
5. The theory of Symptom Formation.

This traditional list of the parts of psychoanalytic theory may not be exhaustive; and it may give the quite mistaken impression that the various parts are unrelated to one another. But these five parts do jointly cover much of the ground usually regarded as covered by psychoanalytic theory. What we shall do, for brevity, is to examine some very few of the central or characteristic generalizations of each part of the theory. If we find that those from any one part are refutable, we shall have done something to show that this part, at least, of psychoanalytic theory is refutable. If we find the opposite, we shall have done something to show that the part concerned is not refutable.

III. The Theory of Instincts

This claims to discover and classify the basic drives of man. On Freud's *first* Instinct theory, as it is usually called, the basic drives fall into two types — the ego or self-preservative and the sexual.

Consider two different ways in which it could be claimed that this generalization can be refuted.

(a) On Freud's view, an instinct or drive has a "source". This is a "somatic process", but "we do not know whether this process is regularly of a chemical nature or whether it may also correspond with the release of other, e.g. mechanical, forces."³ Now when we do come to uncover relevant physiological processes, it is possible that we shall discover that whereas some of the ego instincts (e.g. thirst and hunger) have "a source", others (e.g. flight from danger) have not. Or, what is more likely perhaps, we shall discover that the whole notion of "a source" is far too naïve to cover the complex facts about the bodily origins of instinctual activity.⁴ If we discover either of these things we will succeed in forcing analysts to modify the theory. So, it could be said, it is possible in principle to refute this generalization of the Instinct theory.

(b) New clinical material may turn up which it is difficult to order in terms of the theory, and which suggests a different classification of instincts. It is well known that something like this is what actually happened. Freud found himself faced with the phenomenon of narcissism, as well as with the puzzle of masochism and a type of aggression that was apparently not a response to frustration. He decided that these phenomena constituted a good objection to his first theory of instincts. Accordingly he revised it and replaced it by what is known as the second theory. In this, the basic drives are grouped into the Death and the Life Instincts, the latter being subdivided into the self-preservative and the sex drives.

But though this material forced Freud to modify his first theory, is this an instance of refutation? There are two obvious objections to supposing that it is.

(i) The clinical material or data are obtained by the use of psychoanalytic method. This method is suspect on the ground that it helps to *manufacture* the data, unlike an accepted scientific method that merely uncovers them. In other words, the validity of psychoanalytic method has not yet been established. We do not know, therefore, what weight to attach to the claim that by means of it certain data have been discovered. Consequently, when Freud, or other analysts, apparently uncover material that conflicts with a generalization of the theory, we are uncertain whether we can count this material as a negative instance, and hence as refuting the generalization and the theory. The complications here are obviously immense.

(ii) But it is clear that, whatever be the truth about (i), the “discovery” of new, conflicting material by Freud, or any other analyst, does constitute *an* objection to his first Instinct theory, even if it is not a conclusive objection for the reason just mentioned under (i). Well, then, is it correct to say that the new material “refuted” Freud’s first theory? We are inclined to say “Yes, it is”, because the new material cannot be fitted into the theory. This led Freud and Freudians generally to agree that the first theory was unsatisfactory, even though most analysts did not accept Freud’s second theory, or agree on what to accept instead. But we may be inclined to say “No, the new material has not refuted the first theory”. If so, the reason behind our inclination may be that we are accustomed to talk of data as refuting a hypothesis, and not as exhibiting the inadequacy of a classificatory scheme.

It is now clear, however, that our difficulty hinges round the word “refute”, and not round this part of psychoanalytic theory. In the context we are dealing with, the word “refute” functions as a tool of philosophical art, and the way we answer the question will depend partly on the way the word is used in our own special brand of the philosophy of science. Moreover, the fact that we hesitate over this case because we do not know whether to describe it as a case of “refutation” — this fact suggests that the ways in which generalizations are supported by data are more complex than we are led to suppose by the simple demand for refutation. For this reason alone, it may be quite misleading to demand *simpliciter* of a theory whether it can be refuted or not. Hence, even when we limit ourselves to a demand that the *parts* of psychoanalytic theory should be refutable, we may be making a demand that is misleading and more or less inappropriate.

IV. The Theory of Development

I shall interpret this part of psychoanalytic theory widely to include the theory of character formation, as well as Freud’s account of erotogenesis and sexuality. The essential characteristic of this part of the theory is that the generalizations it contains are universal and relatively straightforward in nature. This is understandable, since Freud is concerned here to give us the stages of mental growth and the antecedents of characters of different types. The natural way to express his “discoveries” here is to put them in universal form. I shall pick out, very arbitrarily, a few examples of his generalizations.

1. "A second pregenital phase is that of the sadistic-anal organization (of sexual life)."⁵ Let us express part of this generalization in a more usual and convenient form. "All children go through an anal-sadistic stage." What this means, at least in part, is that all children go through a state when their chief sexual interest is anal pleasure.

Is this refutable? One hesitates because it is so vague. What is a "stage"? How does one decide that their interest in faeces is "sexual"? How does one discover that what a child is really interested in, in expelling its faeces, is anal pleasure? How does one set about determining the relative strengths of the oral, anal-sadistic and phallic interests? It is tempting to conclude that the generalization is *so* vague as to be irrefutable. But is there much point in falling for this temptation? For if we do fall for it, are we doing much more than advertising our personal decision to put a generalization with this degree of vagueness beyond the limits of the refutable? And is not such a decision a bit arbitrary in view of the fact that all sorts of empirical considerations obviously do have a bearing on the truth or falsity of this generalization? Would not a careful programme of Gesell-like observation on a large sample, from different home-training regimes, do *something* to support or upset the generalization? And can we not imagine a programme of investigation, the results of which could make us inclined to say that the generalization *had* been refuted? I think we can imagine such a programme.

Consider the other, related generalization: "All children go through an oral stage." As this activity is the manifestation of an instinct, it must have an aim; and, in accordance with the general theory of psychosexuality, this is the attainment of pleasurable, auto-erotic stimulation from the erogenous zone of the mouth. It is clear, therefore, that one way of testing this oral generalization is to try to discover whether infants have a drive to non-nutritional, or pleasure, sucking. Various workers have investigated this question. The general finding is that animals and infants do give good evidence of non-nutritional sucking, which goes to support Freud's generalization. However, it is possible to explain this sucking as an outcome of learning, in which case it would not be the manifestation of an instinct, sexual or otherwise, and Freud's generalization would be refuted.⁶ It does not seem beyond the wit of man to devise further work to help to decide between these two alternatives. Indeed, this further work may have already been done — I am rather ignorant on these matters.

Now let us return to the generalization about the anal stage. What would help here is some work which would show whether infants and animals indulged in pleasure defecation; and if they did, whether this was an innate or learned drive. Clearly, there is nothing logically absurd about such a programme of investigation; and indeed it may not be beyond us at the present time to design an actual programme of this sort, if it has not already been done and carried out. The important point, however, is that, if such a programme showed that the interest in anal pleasure was learned, we would be strongly inclined to say that Freud's generalization had been refuted. We would say this about it in spite of it being so depressingly vague.

Next let us look quickly at another generalization.

2. "All little girls have penis envy and wish to be boys."⁷ It seems clear that the direct observation of little girls fails to support this generalization,⁸ as most observant parents will agree. Orthodox analysts will probably reply that this negative evidence is not conclusive, because girls who do not show overt signs of penis envy have concealed it from view or repressed it. Their evidence for saying this stems from their use of psychoanalytic method in the analysis of adults and in play therapy with children. To assess the weight of this evidence we have to determine the validity of psychoanalytic method. In the absence of such an enquiry, the truth-value of the generalization will remain uncertain. But have we any grounds for asserting that it is not refutable *in principle*? Surely none whatever. Of course, it may be difficult to refute it in practice, and for reasons connected with the difficulty of doing satisfactory research in this field. But part of the business of scientific workers is to overcome the practical difficulties in their way. In any case, these difficulties constitute a fact about the sociology of psychoanalysis, not a fact about the logic of psychoanalytic theory.

I shall now glance at two generalizations from the psychoanalytic view of character formation. An essential part of the story about the Oral Character can be stated as follows.

3. "(i) All people can be placed on a continuum into two types — the self-assured optimists at one end and the insecure pessimists at the other; and (ii) the position of any one person on this continuum is determined by the gratification or frustration he experienced at the breast."⁹

Now this generalization asserts that there is a determining or causal connection between certain experiences at the breast and adult char-

acter; and in so doing presupposes that there is a correlation between these experiences and adult character. Goldman¹⁰ discovered that people could be rated in the way the first part of the generalization alleges; and that there was a small but significant correlation between late weaning and optimism, on the one side, and early weaning and pessimism on the other. In short, she discovered that there was in fact a correlation of the sort presupposed by psychoanalytic theory. Suppose, however, that she had discovered something else — either that there was *no* correlation here at all, or that the correlation was the *reverse* of what she actually discovered. Her results would then have conflicted with generalization 3. Would this then have counted as a refutation of this generalization? Does it much matter what we would say here? It is quite clear that, whether we would call this a case of refutation or not, this generalization of psychoanalysis is open to empirical support and rebuttal.

An essential constituent of the psychoanalytic doctrine of the Anal Character is a generalization that can be expressed as follows:

4. "Fixation at the anal-erotic stage of instinctual development goes to produce or determine the 'anal character' traits of parsimoniousness, orderliness and obstinacy."¹¹

This generalization implies a correlation between severe and rigid bowel training and the possession of these traits. Some studies have been made of this question, but, to my knowledge, the results have been inconclusive. It has not even been firmly established that there is such a cluster of traits as the anal character, let alone that such clusters, where they exist, are correlated with severe bowel training.¹² Some of us may be tempted perhaps to think that the uncertain status of this generalization is inevitable; and, if it is inevitable, then it is not refutable. But, as soon as one looks a little closer at it in its context in psychoanalytic theory, and in the relevant body of scientific work, it becomes clear that its present uncertain status neither entails nor implies that it is irrefutable. The temptation to think otherwise dissolves under the pressure of contextual fact.

The upshot about this part of psychoanalytic theory — the theory of Development — is that it is open in principle to scientific investigation, and that it is open to it in practice as much as the complexities and other difficulties of the field permit. It is worth noting that psychologists have not hesitated to investigate this part of psychoanalytic theory, even though they have recognised that their relatively crude methods may not be able to deal with the subtleties of the phenomena

involved. We have to remember, however, that almost any negative finding — such as that over female penis envy — can be countered by analysts claiming that it is necessary to postulate such girlish envy in order to account for the material thrown up in analysis. The strength of this reply can only be resolved, presumably, by settling the validity of psychoanalytic method. All of which suggests that this part of psychoanalytic theory opens up a most interesting field for scientific research.

V. The Theory of Psychic Structure

This part of psychoanalytic theory is quite different in logical character from the two parts we have just glanced at. Its key words (e.g. id, ego, unconscious etc.) appear to function in two different ways. They function as nouns, and they also function as adjectives, or in ways that are implicitly adjectival in character. As nouns, they go to make up a story that gives us “the topography of the mind”; as adjectives, they appear in expressions such as “ego functioning”, “super-ego development”, “unconscious fears”, and so on.

Let us first consider their use as nouns — in other words, the psychoanalytic theory of mental topography. It is well known that Freud spoke of the mind as having parts (e.g. the Ego, the Unconscious), containing mental elements charged with energy which are held back by barriers, and so forth. What are we to make of this mystifying talk? Is it just a piece of reifying mythology? Freud himself suggested that we regard it as a *façon de parler*, to be rejected as soon as we find something better.¹³ Perhaps a more fashionable way of regarding it today is to look on it as providing a model of mental functioning.

Suppose we do regard Freud’s topographical talk in this light. At once we remove its dark and reifying aspects. For we are now only speaking of the mind *as if* it had parts etc.; and in so doing we admit, more or less explicitly, that it does not really have them. However, it then follows that it is inappropriate to ask of this talk whether it is true or false. Hence it is also misleading to ask whether the statements that comprise it are true or false; and therefore misleading to ask whether they can be *shown* to be true or false — whether they can be supported or refuted. The concepts of truth and falsity, refutability and irrefutability do not apply in a straightforward way to the statements of his topographical theory.

It seems therefore as if the critics of psychoanalytic theory would be

right if they picked on this part of the theory as being irrefutable. But how serious is this defect? Is it really the sort of irrefutability that the critics want to establish? I doubt whether it is serious, or the sort of defect that the critics are interested in. The fact that psychoanalytic theory uses a model makes it no worse, *prima facie*, than any other theory in science that uses one. If we are going to object to psychoanalytic theory *just* because it uses a model, then we should also have to object to any other theory in science that uses one. Which is absurd.

But though we cannot ask whether Freud's topographical talk is true and refutable, we can ask whether it is valuable or not. "How useful was and is it? Are there circumstances or developments in which it would cease to be useful and require modification?" The short answer to these large questions seems to be as follows. Analysts do need to talk in a shorthand way about the total functioning of the individual; and it just seems to be a fact that they find it convenient to do so in spatial terms. The particular model Freud provided was of enormous service to them at the time. Since he wrote, however, various things have led analysts to canvass alternative models more or less explicitly. Thus the work of Fairbairn and Klein¹⁴ has led some analysts to speak of "the internal objects that the infant incorporates", and to stress the importance of this and other related performances. A model constructed in accordance with these ideas would be rather different from Freud's. Again, the clinical experience of Laing¹⁵ with schizophrenics has led him to stress that they lose their sense of self-identity, and that it is essential for us to grasp this fact to understand their condition. Clearly if we were to attempt to construct a model that used this idea, and that was general enough to cover the neuroses as well, we would produce one very different from Freud's. For his is suggested by his experience with the neuroses, and the Self does not figure in it. Here, then, it could be alleged, are two developments which show where Freud's model is somewhat unhelpful, and in need of modification. It is not difficult to think of further developments and circumstances that would suggest still other modifications of Freud's model.

But though this model had, and has, its uses, is it a *good* one? The answer is: certainly not! It is a very bad one — it has almost everything a model should not have. It produces the most misleading pictures about this part of psychoanalytic theory and about the theory as a whole. Indeed, it has been so misleading as to produce near apoplexy among some American students of behaviour.¹⁶ When we

look into it carefully, it crumbles under our scrutiny because it has not been worked out with sufficient care and precision. We cannot use the model to derive any predictions by reference to which it can be tested. In these respects it is quite different from the models currently employed by psychologists. If we wish to exhibit how inadequate it really is, we can do no better than compare and contrast it with some examples of models under current discussion in the psychological world.¹⁷

Now let us consider the adjectival use of the words such as "id" and "unconscious". When the words are used in this way, they do not function in model talk at all. This is evident from one fact alone, namely, that whereas Freud was prepared to regard their use in a model language as a *façon de parler*, and accordingly was prepared to sacrifice them in favour of any better alternative, he was not prepared to regard them in their adjectival use as mere ways of talking. On the contrary, in this use, he was convinced that they were indispensable, and for the reason that they expressed concepts that were valid, and hence went to form statements that were true.

Let us look at three examples of statements from the theory in which these words function as adjectives.

5. "An individual exhibits both ego and super-ego functioning." (This is an abbreviated version of a much longer story.)

6. "The super-ego is the heir of the Oedipus complex."¹⁸ The use here of "super-ego" as a noun enables Freud to say with dramatic brevity that the development of super-ego functions culminates with the resolution of the Oedipus complex.

7. "...unconscious mental processes exist."¹⁹

Consider 5. This statement exhibits Freud's way of "dressing up" a truth of common sense. We all know that, for example, we do try to cope realistically with the world, that we do try to deal with threats to our security, and so on. We all know, too, that we are controlled by our consciences, and so forth. Freud dressed up these truths of common sense when he came to incorporate them within his general theory of mental functioning. Is statement 5 refutable? Presumably it is — but in the sort of way or ways characteristic of the generalizations of common sense. Thus, it is possible perhaps to imagine ourselves being shown that the two concepts — of ego and super-ego functions — are basically misguided and have no application at all. For it is possible that when we really have achieved a scientific understanding of the relevant phenomena, we shall discover that these concepts of

ego and super-ego activity are so crude and naïve that we have to reject them as inapplicable. Or we *could* object here and now and say that 5 is false because it overlooks a very important class of individuals, namely psychopaths. On one view of psychopathy, some psychopaths are people without super-ego development. If this view is correct, then 5 is false. But though we could say this, *would* we say it? I doubt it. For in producing this exception we bring out the importance of statement 5. Indeed, we bring out that we use 5 to explain this very exception to it, namely the class of psychopaths. Moreover, we do not withdraw our commonsensical generalization that, for example, "subject peoples resent their inferiority" by being told that some black servants in South Africa love their masters and do not resent their station in life. So, also, we do not withdraw the dressed-up, but commonsensical, generalization in 5 when we are presented with an exception. It is for these reasons that we would not use the class of psychopaths *as* an exception. The upshot, therefore is that 5 is refutable, but that it is odd to raise the question of refutation about it — in the way that it is odd to raise this question about some of the generalizations of common sense.

Take 6. This is a straightforward generalization about development. We have commented on this type already. It is worth remembering that 6 has been challenged by Klein in the light of her evidence from play therapy.²⁰

And 7. "... unconscious mental processes exist." This is a more complicated assertion. It resembles 5 in that it is saying something quite commonsensical. Unconscious wishes etc. have been part of our common sense knowledge of man since Aesop's report about the fox and the grapes. Where 7 is uncommonsensical is in the fact that Freud asserted it at all. He did this because he was concerned to maintain that a large number of certain *specific* unconscious mental processes were at work in us and with important results. (E.g. that unconscious Oedipal wishes occurred with important consequences for mental health later on in life.) In maintaining this view he flew flat in the face of common sense and of the science of his day. What is also uncommonsensical about 7 is the use to which he and others put it — namely in treatment. It was this whole new context in which 7 now functioned that made this commonsensical generalization so important and interesting. Because 7 is a common-sense generalization, it is refutable in the way that is characteristic of the generalizations of common sense. Just because of this, there is something odd about asking: "Is 7 refutable?",

and we do not naturally raise this question. What we *do* ask quite naturally is whether a generalization about some *specific* unconscious process or state is true or not — for example, the generalization about unconscious Oedipal wishes. In asking such a question we presuppose that these generalizations about specific unconscious processes can be refuted in principle. We have discovered no reason so far for saying that this presupposition is false in any way that matters.

VI. The Theory of Mental Economics

The character of this part of psychoanalytic theory is again very different from the other parts we have touched on. This part, in the orthodox view, is concerned to describe the ways in which the individual deals with the mental excitation to which he is subject and reduces it to an optimum level. Two of the essential features of this part of the theory seem to be as follows. (i) It contains certain concepts such as Repression, Regression, Projection, Identification, etc. (Quite often these words are also said to name Mechanisms of Defence.) (ii) These concepts do not appear typically in generalizations that are universal or statistical in form; but appear typically in generalizations with a form rather like this: "When so and so happens, the individual tends to, e.g. repress ..." Thus, when Fenichel discusses these concepts he offers a number of scattered generalizations about them, and the key words in these generalizations are words such as "tend to", "may", "in general" (a non-numerical use), and "prevalent" (a non-numerical use).²¹

This part of psychoanalytic theory is confusing and obscure in various ways. There is no agreement on what set of concepts to include in this part of the theory. There is no one, generally used, formulation of these generalizations. To say that these concepts appear typically in generalizations that are "tendency statements" is to say something in need of further analysis.²² Though the typical generalizations in which they appear may be of this form, some also appear in universal ones — especially those connected with the development of the person, and the ages and stages at which various mechanisms of defence are first or predominantly used. It is not at all clear how the different parts of this part of the theory fit together. It could be argued that they fit together very loosely, and that the connective "and" is about all that conjoins them. On top of all this, the concepts themselves are vague and obscure; and few analysts have ventured to help themselves

and others by an exercise in clarification. Yet in spite of all this, the theory of Mental Economics or Defence is a critically important part of psychoanalytic theory. This becomes very apparent when psychoanalytic theory as a whole is applied to a given individual in the attempt to understand him and to develop clinical expectations about him. Indeed, some contemporary analysts might be tempted to say that much of the psychoanalytic theory we have already considered is rather dead wood and of uncertain value; but that the theory of Mental Economics is the part of psychoanalytic theory that is most alive and of growing importance.

Is this part of the theory open to refutation? In view of its confused and unsatisfactory state, anyone may be forgiven for thinking that the answer is obviously negative, and that we need pursue the matter no further. But let us glance at some generalizations.

8. "Where aggressive behaviour towards the external sources of frustration is prevented, this behaviour may be displaced."

Neal Miller constructed a situation experimentally between two rats in which, in the presence of a doll, aggressive behaviour against each other was rewarded. He then removed one rat, and the aggressive behaviour was then directed against the doll.²³ What is the relation between this finding and generalization 8? Suppose Miller's finding had been negative — that, e.g., the rat started to bite and apparently attack itself and not the doll. This would have forced us to ask various questions. For example, is this failure to obtain an analogue of displacement due to the vast differences between rats and humans? Or is it due to some defect in the design of the experiment? Or has the experimenter just misunderstood the concept of displacement in designing this experiment? We would have been forced to put these questions because this negative result would have been relevant to the truth of generalization 8. Suppose we had then investigated the problem in various different ways and turned up with negative results all along the line. We might then have found ourselves saying: "The only interpretation we can put on these results is to say that aggressive behaviour is not displaced. What we find is that the aggressive behaviour is apparently self-directed, where it is directed at all." It is quite conceivable that we should have discovered these negative results. If we had discovered them, they would have opposed the "may be displaced" of generalization 8 with the finding that the aggression "does not appear to be displaced at all". It is quite correct to say that such findings would have disconfirmed or

failed to support the generalization 8 in some measure. Because of this, it is also correct to say that Neal Miller's actual findings lent the generalization some small measure of support.

9. "Boys tend to identify with their fathers in a more clear-cut and decisive way than do girls with their mothers."²⁴ Blum devised a projective test, using a series of cartoons or pictures about a dog Blacky, to investigate a number of psychoanalytic generalizations.²⁵ He discovered that there was a tendency of the sort asserted in 9. He also discovered, however, that females tend to have a motherly rather than a fatherly super-ego. This finding conflicts with psychoanalytic theory, if the latter be taken to include the generalization that both males and females tend to develop fatherly super-egos.²⁶ In other words, a tendency generalization of psychoanalytic theory is supported by finding the alleged tendency on enquiry, and it is not supported when the alleged tendency is not found but some conflicting tendency instead. It is apparent, however, to the discerning eye that fruitful work on identification may only be possible after further clarification of the concept itself. In recent years psychologists have turned their attention to this logical task on several of the concepts concerned, including that of identification.²⁷ This gain in clarity is one of the undoubted benefits that has accrued from the attempts of psychologists to investigate the generalizations of psychoanalysis.

10. "When an individual is subject to frustration, there is a tendency for him to regress to an earlier level of either libidinal or ego development."²⁸

An oft-quoted investigation in this field is that of Barker, Dembo and Lewin.²⁹ These workers arranged a play situation in which children were subject to frustration and then showed what looked like considerable ego-regression. Like other studies of the concept of regression, this work raises quite acutely the question of how close is the experimentally produced analogue of regression to the regression that the analysts are really talking about. The less close the analogue, the less weight is to be attached to the experimental results, positive or negative; and conversely. To arrive at even a provisional decision about this question is a highly technical business; and the technicians are apt to be influenced in their judgements by various non-rational considerations — such as their romantic or sceptical attitude to objective work in psychology, their readiness to work patiently to pick up the crumbs of scientific discoveries, and so forth.

11. "Unsatisfied wishes are the driving power behind phantasies; every separate phantasy contains the fulfilment of a wish, and improves on unsatisfactory reality."³⁰

Suppose we give this statement a strong interpretation as a universal generalization. The kernel of it is that "All phantasies are wish fulfilling." We may be tempted to say two different things about this statement. That it is obviously true, being only a matter of common sense; and that it is obviously not the sort of thing we can investigate scientifically at all. But the fact is that not all people have believed it,³¹ and it is open to scientific enquiry. Thus, Feshbach³² argued that if a social situation were to arouse hostility in people, then the expression of their hostility in phantasy should reduce somewhat the hostility they felt towards this situation. On investigation Feshbach found this hypothesis was correct. It is natural to say that this finding supports generalization 11, and that if the contradictory finding had been made, it would have counted against the generalization. It is hardly necessary to say that, and why, Feshbach's work is not conclusive. The inconclusive outcome of a single experiment is a common feature of work in science, and not one restricted to psychology alone.

I have now run through a very inadequate sample of psychological enquiry in the field of Mental Economics or Defence. Do we *still* feel inclined to say that the generalizations in this part of psychoanalysis are irrefutable? Probably not. Obviously this inclination is a bit misplaced. Whatever may be wrong with this part of psychoanalytic theory — and it is evident that there is a great deal the matter with it — it is not its alleged irrefutability. When we look into the details here, the sharp dichotomy between what is refutable and what not ceases to be important and relevant.

VII. The Theory of Symptom Formation

This part of psychoanalysis is concerned to tell us how the working of the mental system goes wrong. It includes statements of different sorts. It gives us a way of talking about neurotic conflict and symptom formation as a whole. It tells us about conditions that determine or contribute to the manifestation of mental disorder. It gives us descriptive accounts of the standard neurotic and character disorders, along with descriptions of the aetiology and psychopathology of the typical cases of each type of disorder. Let us glance at some examples of statements of these different sorts.

12. "The neurotic conflict takes place between the ego and the id."³³

Now this statement points to the way in which psychoanalytic theory is used to cover the facts of mental disorder. In this and many other related statements, Freud and others are applying their general scheme, or way of talking, to the pathological material. It follows, therefore, that it is not appropriate to plunge in at once and ask whether statement 12 is refutable. The appropriate question to ask is whether this statement represents the correct application of psychoanalytic theory to the pathological material. Is this the right way to talk about neurotic conflict inside psychoanalytic theory? It may seem that, to answer this question, we shall have to confine ourselves to a tortuous linguistic exercise inside psychoanalytic theory itself. But interestingly enough, this may not be the case, as the following discussion illustrates.

When we use psychoanalytic theory to describe neurotic conflict, a critical question that arises is: what role does the super-ego play in it? A few years ago Miller and Dollard argued³⁴ that the neurotic conflict is manifested when the super-ego in alliance with the ego becomes too strong for the id. On the other hand, Mowrer³⁵ argued that neurotic conflict was manifested when the id and the ego became jointly too strong for the super-ego. Eysenck has touched on this question in the course of factorial and objective studies of personality. He has suggested³⁶ that the disputants are concerned here to describe two different types of patients who fall along a personality dimension or continuum of Introversion and Extraversion. Miller and Dollard are describing in psychoanalytic terms the symptom pattern exhibited by introverted patients; Mowrer the symptom patterns exhibited by extraverted. We need not ask whether Eysenck's suggestion is correct and, if so, whether it is sufficient to resolve the argument. It is interesting to us because it brings out a connection between the psychoanalytic talk about ids and egos and psychological fact; and it shows that the logical tie is closer than we are apt to imagine.

13. "Anxiety (in the case of Anxiety Hysteria — B.A.F.) over being eaten or over being bitten may be a disguise for castration anxiety."³⁷

This generalization springs from the analytic view that children who have had difficulties at the oral stage are more likely to exhibit castration anxiety. Hence when a person has castration anxiety this fact may come out in regressive form in symptoms of an oral kind. Now for 13 to be true, it must be true that disturbances in the oral

stage are correlated with castration anxiety. Using the Blacky test, Blum found that there was a low positive correlation between the two.³³

14. "A necessary condition for the production of male homosexuality is the fixation of erotic needs on the mother."

This is a summary of part of Freud's view on the matter.³⁹ It is worth noting that subsequent analytic experience with homosexuals has led analysts to modify this generalization by (roughly) restricting its validity to a certain type of homosexuality.⁴⁰

Generalization 14 resembles a number of other generalizations in this part of the theory. These state that a certain condition is either a necessary or a contributory causal antecedent to the development of a certain mental disorder. Perhaps the best known of these is the generalization about maternal deprivation and its effects. The difficulties that have arisen in the attempts to investigate this generalization are typical of some of the difficulties facing scientific enquiry in this sea of subtle data.⁴¹

15. "(In Anxiety Hysteria — B.A.F.) the unconscious impulses belong to the genital incestuous phase of childhood (the Oedipus situation), and the punishment dread is genital castration or mutilation."⁴²

There is a form of neurosis traditionally called "Anxiety Hysteria"; and generalization 15 is part of the orthodox account of the aetiology and psychopathology of this condition. Now it is a fact that not all analysts obtain this aetiology and pathology in all cases of anxiety hysteria. For example, Dicks has reported⁴³ a small sample of cases of this sort, some of which diverged more or less from the orthodox picture. He made the point that the classical cases of revived Oedipal conflicts "do undoubtedly occur", and these "led the Freudian school to a premature generalization which has of late been partly withdrawn". Of course, counter-evidence from psychoanalytic experience, such as Dicks has produced, raises the interesting and critical question: What supporting and what refuting force should we give to such evidence? Clearly Dicks' experience counts to some extent against generalization 15. But how much? This is a large question which takes us away — as we have already noted — into the whole unresolved issue about the validity of psychoanalytic method. However, in spite of this uncertainty it is still open to analysts and others rationally to discuss and evaluate the force of the data that their own method produces.

VIII. Conclusion

Well, is it logically possible to refute psychoanalytic theory? Do we still feel moved to ask this question?

It is quite clear that the question poses an important and salutary challenge. It forces us to attend to the logical character of psychoanalytic theory. How far is this like and unlike a scientific theory? The question brings analysts to a full stop with a jolt, and obliges them to think about the presuppositions of their work, to take note of the weakness of the appeal to "psychoanalytic experience", to try to make their fuzzy discourse somewhat less fuzzy.

But this challenge is also very misguided. It suggests that there is a sharp line between the refutable and the irrefutable, which is quite misleading, both in general and when considering the nature of psychoanalysis. When one looks into the details of psychoanalytic discourse one finds it impossible generally to say whether a generalization is refutable or not, and the question does not really seem to matter. To ask whether psychoanalytic theory is refutable is also a bit silly in view of the fact that the scientific study of personality is so much concerned with the business of investigating the truth of the generalizations of the theory. Of course, one can *lay down* a tight set of criteria that have to be satisfied before a theory can count as refutable; and we can make these criteria such that psychoanalytic theory has then to be rejected as irrefutable. But what is the point of such legislation? Furthermore, to throw down the challenge of refutability reinforces the idea that psychoanalysis is just a wonderful representation of the facts — like some remarkable but fanciful picture. But it is clear that, in a very important way, the theory is a *very bad* representation of the facts, and not wonderful at all. The challenge of refutability also reinforces the idea that the whole story may be myth, rather like the pre-scientific myths of earlier days. But when one looks into the details of the discourse and sees the close connections between it and empirical fact, the mythical character of the story vanishes, and it becomes a theory that is an approximation to the truth. But, clearly, *no brief* characterization of psychoanalytic theory will do; it is far too complex and diversified for any short description to work. Still, if we *must* have a brief characterization then let us remember the origins and point of the discourse. Freud was forced to construct a story that purported to cover the whole or total field of fact he was meeting in his work. In being forced to construct a "total" narrative, he gave us, naturally

enough, a very crude and provisional story. It is a remarkable story because of the novelty and importance of the suggestions it contains, and the range and genius it exhibits. But being a provisional story, it is a defective one. Obviously it is up to analysts and workers in cognate fields to try to improve the situation, and by the application of scientific methods to produce a better narrative. I hope that this paper has made it clear that an enterprise of this sort has been under way for some little time.

NOTES

- ¹ Popper, K. R. (1957) in *British Philosophy in the Mid-Century*, C. A. Mace (ed.). Allen and Unwin, London.
- ² Moore, G. E. (1955) "Wittgenstein's Lectures in 1930—33". *Mind*, 64, No. 253.
- ³ Freud, S. (1915) "Instincts and their Vicissitudes". *Collected Papers*, Vol. IV. Hogarth Press, London, 1925.
- ⁴ Cf. Beach, F. A. "Instinctive Behaviour: Reproductive Activities", in *Handbook of Experimental Psychology*, S. S. Stevens (ed.). Chapman and Hall, London, 1951.
- ⁵ Freud, S. *Three Essays on the Theory of Sexuality*. Imago Publ. Co., London.
- ⁶ See, for example:
 Levy, D. M. (1934) "Experiments on the sucking reflex and social behaviour of dogs". *Amer. J. Orthopsychiat.*, 4, 203—224. Roberts, E. (1944) "Thumb and finger sucking in relation to feeding in early infancy". *Amer. J. Dis. Child*, 68:7—8. Halverson, H. M. (1938) "Infant sucking and Tensional behaviour". *J. Genet. Psychol.*, 53, 365—430. Sears, R. R. and Wise, G. W. (1950) "The relation of cup feeding in infancy to thumb-sucking and the oral drive". *Amer. J. Orthopsychiat.*, 20:123—138.
- ⁷ Freud, S. op. cit.
- ⁸ Valentine, C. W. (1942) *The Psychology of Early Childhood*, Ch. 18. Methuen, London.
- ⁹ Fenichel, O. (1945) *The Psychoanalytic Theory of Neurosis*, Ch. 20. Kegan Paul, London.
- ¹⁰ Goldman, F. (1948) "Breastfeeding and character formation". *J. Personality*, 17, 83—103.
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- ¹¹ Freud, S. (1908) "Character and Anal Erotism". *Collected Papers*, Vol. 2 (1924). Hogarth Press, London. Fenichel, O. op. cit.
- ¹² Sears, R. R. (1936) "Experimental Studies of Projection: Attribution of traits". *J. Soc. Psychol.*, 7, 151—163. Blum, G. S. (1953) *Psychoanalytic Theories of Personality* Ch. 8. McGraw-Hill, New York.
- ¹³ Freud, S. (1935) *An Autobiographical Study*, Ch. III. Hogarth Press, London. In contrast Freud strongly repudiated Janet's view that "unconscious" as an adjective was just a *façon de parler*. See below, and Freud op. cit. Ch. III., and *Introductory Lectures on Psycho-analysis*, Lects. 19 and 17. (Rev. Second Ed. 1929) Allen and Unwin, London.

- ¹⁴ Fairbairn, W. R. D. (1952) *Psychoanalytic Studies of the Personality*, Tavistock, London. Klein, M. *et al.* (1952) *Developments in Psycho-Analysis*. Hogarth, London.
- ¹⁵ Laing, R. D. (1960) *The Divided Self*. Tavistock, London.
- ¹⁶ See, for example, Skinner, B. F. "Critique of Psychoanalytic Concepts and Theories", in Feigl, H. and Scriven, M. (1956) *Minnesota Studies in the Philosophy of Science* Vol. I. Univ. of Minn. Press, Minneapolis.
- ¹⁷ For example Deutsch, J. A. (1953) "A New Type of Behaviour Theory". *Brit. J. Psychol.* 44, 304—17; and Broadbent, D. E. (1958) *Perception and Communication*. Pergamon Press, London.
- ¹⁸ Freud, S. (1927) *The Ego and the Id*. Hogarth, London.
— (1924) "The Passing of the Oedipus-Complex". *Collected Papers* Vol. II, Ch. 23. Hogarth, London.
- ¹⁹ Freud, S. (1929) *Introductory Lectures on Psycho-analysis*. Rev. Second. Ed. Eighteenth Lect. Allen and Unwin, London.
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- ²¹ Fenichel, O. *op. cit.* Ch. 9.
- ²² Farrell, B. A. (1949) "Causal Laws in Psychology". *Arist. Soc. Suppl.* Vol. 23. Braithwaite, R. B. (1953) *Scientific Explanation*. Cambridge.
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- ²⁶ Fenichel, O. *op. cit.* Ch. 6.
- ²⁷ Stoke, S. M. (1950) "An inquiry into the concept of identification". *J. Genet. Psychol.* 67, 163—189. Gray, S. W. and Klaus, R. (1956) "The assessment of parental identification". *Genet. Psychol. Monogr.* 54, 87—114.
- ²⁸ Cf. Glover, E. (1949) *Psycho-analysis*, Second Ed. Staples Press, London. Fenichel O. *op. cit.* Ch. 9.
- ²⁹ Barker, R., Dembo, T., and Lewin, K. (1941) "Frustration and Regression: an experiment with young children". *Studies in Topological and Vector Psychology II. Univ. Iowa Stud. Child Welf.*, 18, No. 1, 1—314.
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- ³³ Fenichel, O. *op. cit.* Ch. 8.
- ³⁴ Dollard, J. and Miller, N. E. (1950) *Personality and Psychotherapy*. McGraw-Hill, New York.
- ³⁵ Mowrer, O. H. (1953) *Psychotherapy: theory and research*. Ronald, New York.
- ³⁶ Eysenck, H. J. (1957) *The Dynamics of Anxiety and Hysteria*. Routledge and Kegan Paul, London.
- ³⁷ Fenichel, O. *op. cit.* Ch. 11.

³⁸ Blum, G. S. op. cit.

³⁹ Freud, S. (1910) "Leonardo da Vinci and a memory of his childhood", *The Standard Edition of the complete psychological works of Sigmund Freud*, ed. James Strachey, Vol. 11. Hogarth Press, London, 1957.

⁴⁰ For a brief review of this question, see Wohl, R. R. and Trosman, H. (1955) "A Retrospect of Freud's *Leonardo*, and Assessment of a Psychoanalytic Classic. *Psychiatry*, 18, 27—29.

⁴¹ For a sceptical review by an outsider see Barbara Wootton (1959) *Social Science and Social Pathology*, Ch. 4. Allen and Unwin, London.

⁴² Glover, E. op. cit. Ch. 10.

⁴³ Dicks, H. V. (1947) *Clinical Studies in Psychopathology*, Second Ed. Ch. 2. Edward Arnold, London.

ETHICS, METAPHYSICS, AND PSYCHOANALYSIS*

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An attempt is made to point out some of the moral presuppositions and implications of psychotherapy in theory and in practice. In the various forms of dynamically oriented psychotherapy, the setting of goals and decisions concerning maturation and health have value components. Even in psychoanalysis in its strict sense, decisions made in accepting or rejecting contracts and determination of the terminating point of analysis are value laden.

Finally, in the theorizing of Freud and others about psychotherapy moral positions are assumed.

I. Introductory Definitions

In its most permissive, if not amorphous, sense, 'psychoanalysis' refers to the use of psychological (as contrasted to physical) techniques in the treatment of emotionally disturbed people, and to the body of theory that has arisen in the attempt to explain and interpret these techniques. Within this sense, the term applies to all kinds of psychotherapy: inspirational, suppressive, supportive, relationship, and insight therapy. It would not apply, however, to the injunction "Keep a stiff upper lip" to one who has lost his job, nor to the use of electro-shock or anti-depressants in the treatment of a depression. Within this sense, the term applies to many kinds and levels of theories: theories about the trichotomous division of personality into Ego, Superego, and Id, about a system of defence mechanisms triggered into action by the basic personality in the face of anxiety, and theories about the Freudian and Jungian interpretations of the Unconscious. Whether it applies to clerical instructions for the achievement of peace of mind is dubious, but no attempt will be made here to resolve border-line cases.

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I think that it is a mistake to use the word 'psychoanalysis' in the cosmic way just described. The term that I shall use for the application of psychological techniques in the treatment of emotionally disturbed people is *psychotherapy*. *Psychoanalysis* is an intensive form of analytic psychotherapy in which the patient's free associations, dreams, and reports of feelings are interpreted with the aim of making conscious what hitherto were unconscious feelings and motivations. The outcome of a successful analysis is corrective emotional experience, and the ego is strengthened through insight and control. No term has been generally accepted for the theories associated with psychotherapy and psychoanalysis, but I suggest that we use '*psychodynamics*'.

It is notoriously difficult to provide a definition or characterization of metaphysics that will satisfy all or most philosophical camps. Some take metaphysics seriously and others regard it as nonsense. Among those who take it seriously, some believe it to be an autonomous discipline with unique methods and unique results; others think that it is dependent upon some science or group of sciences. There are many alternative interpretations of which these are but samples.

I shall rely, perhaps precariously, on the vague notion that we have of metaphysics from looking at standard books in the history of philosophy. On most accounts, metaphysics is the attempt to give an explanation of the ultimate nature of the universe. In developing metaphysical systems, philosophers found it necessary to compare and evaluate various fields of knowledge. No one can become proficient in all sciences and disciplines, and the field or fields in which a given philosopher becomes proficient makes a discernible difference in the form his metaphysical system takes. For example, Aristotle's work in biology made it impossible for him to accept the dichotomies explicit in Platonism and, as we shall see, Freud's explorations into the genesis of our moral and religious convictions led him to a rejection of any form of supernaturalism.

Finally, when someone conjoins the words 'ethics' and 'psychoanalysis', what sorts of problems does he have in mind? After looking at the literature, I have decided that he is not referring to questions of professional ethics that confront the psychiatrist. It is not unusual in an academic department of psychiatry for the principal administrators to advise residents and junior staff concerning their moral responsibilities. For example, the following injunctions might be issued: (1) Do not discuss your patients at cocktail parties. (2) Keep case histories and other confidential documents locked up when not in use. No one

would deny the importance of such ground rules, but I do not think that the philosopher has much to gain from studying them nor that he is in a position to give advice concerning their formulation or application. It might be the case, of course, that a psychotherapeutically oriented interpretation of the injunctions would cast light upon the compulsive or hysterical character of the administrators in question.

Furthermore, I do not believe that we should be concerned with the employment of psychiatric terms in non-psychiatric contexts. If one philosopher tells a second that he has produced a conclusive proof for the existence of God, the second, who happens to be a naturalistic humanist, might respond: "You've regressed through the oral stage into a vacuum." This probably would be interpreted as a hostile reaction, and for those who are interested in studying the behavior of small groups, it might be important to analyze this kind of response and its implications. Once again, however, I don't believe that the philosopher will make any distinctive contribution to this kind of study.

There are at least three directions that may be taken in examining the relations between ethics and psychoanalysis. One may study the moral responsibility assumed by a psychiatrist in his practice. In some cases, it is alleged, the therapist explicitly recognizes that moral commitments are involved in accepting a patient in therapy, in establishing goals for therapy, and in the therapeutic hours themselves. Suppose that you are choosing a therapist for a patient who is a Negro, a 40-year-old ectomorph, a native of Mississippi, lower middle class, and an enthusiastic member of a store-front church. Suppose that the doctor you decide upon is a 60-year-old white endomorph, an upper middle class New Yorker, and an atheist. The therapist, in employing such concepts as 'adjustment to reality', 'acting out', and 'reality testing', will seldom avoid moral decisions. Secondly, when psychotherapists are theorizing about their practice, their concepts are at times value-laden. Freud's use of the phrase 'penis envy' is an expression of his disparagement of women, and his interpretation of the religious person as regressed is an indication of his disapproval of the Judaeo-Christian religious tradition. Third, the psychotherapist may attempt to give an analysis of the meanings of moral expressions. He is predominantly interested in genetic questions, e.g., when a person says and believes that stealing is wrong, he is responding to the parental disapproval of stealing and is trying to gain parental approval of his attitudes and actions.

II. Interdependence of Philosophy and Psychiatry

So much in the way of clarification or mystification of the terms 'ethics', 'metaphysics', and 'psychoanalysis'. It is ironic to note that both philosophers and psychotherapists in some instances assert that their respective fields are independent of one another. Freud says:

Probably you cannot imagine how alien all these philosophical convolutions seem to me. The only feeling of satisfaction they give me is that I take no part in this pitiable waste of intellectual powers. Philosophers no doubt believe that in such studies they are contributing to the development of human thought, but every time there is a psychological or even a psychopathological problem behind them.¹

He also says that psychoanalysis does not criticize society and has no concern whatsoever with judgments of value.² And philosophers, as I discovered at the New York Conference on Philosophy and Psychoanalysis in 1958, are extremely dubious about the propriety of psychoanalytic interpretations of philosophical theories. Professor Donald C. Williams, in his diatribe against Professor Lazerowitz's psychoanalytic interpretation of the metaphysics of Spinoza and Bradley, says that the relation of psychoanalysis to philosophy is that of the dishpan to the ocean.³

The mutual distrust that some philosophers and psychotherapists have for each other and the insistence that the one field is independent of the other are symptomatic of a basic insecurity. Philosophers frequently feel uncomfortable about their inability to achieve results in the way that their colleagues do. Natural scientists make decisive progress, historians report facts, and linguists obtain data. What are the data of philosophy, if such there be? And does the concept of progress have any application within philosophy? On the other hand, the psychotherapist sometimes wonders how his field compares with the established fields of medicine. He does not produce autopsy reports nor discover brain tumors. A question arises concerning the criteria for confirming and disconfirming hypotheses. Even though one may argue that the success of psychotherapy as a system of techniques is undeniable, a question still remains concerning the relation between theory and practice. Some believe that the theory is ill-contrived, vague, and dispensable.

What I wish to do is to show that psychotherapy and psycho-

dynamics are not independent of philosophy and, in particular, that this is so in the fields of ethics and metaphysics. I am also convinced that philosophy is not independent of psychotherapy and psychodynamics, but I make only a casual attempt to present the evidence for this in this essay. Many of my examples come from Freud, not exclusively because of my antiquarian interests, but also because Freud's influence and importance remain unparalleled.

III. Moral Implications of Psychotherapy

Let us first examine the moral implications of psychotherapy. Consider the case of a university graduate student whose wife has a severe post-partum depression. In the middle of the school year, the couple with their child leave the university community and return to their home environment. The wife responds well to the supportive context of the hospital and psychotherapy, and in a few weeks is well enough to go to her parents' home. The question now arises as to whether this family should return to the university community, in which case the husband could resume his studies, or whether the family should remain in the home environment, in which case his potential career as a research scientist will remain unrealized. The parental figures wish the family to remain in the home environment. They have been dubious heretofore about a grown-up boy who continues to go to school after he has graduated. They want him to get a good, steady job and remain in their social and economic class. If the couple stays in the home environment, probably the girl will feel guilty and her husband angry. On the other hand, if they return to the university community, which is associated with the illness, there is a good chance that the husband will feel guilty and the wife angry. These are some of the variables involved, but of course there are many more factors which I leave unmentioned.

What is the therapist to do in this situation? Let us assume that the wife is an out-patient now and that he has seen the husband alone a few times and that he has seen both together on more than one occasion. Of course, he will say that the decision is one that the couple must make, but surely it is obvious that his support, one way or the other, will be a major factor in the decision that is reached. His attitude and counsel will be guided by his conception of which is the better life for the couple to pursue.

Why is it that some psychotherapists are inclined to deny that there

are moral implications in their practice? First, there is the myth which is conveyed effectively by the following parody of the alleged neutrality of the psychotherapist:

A distressed patient appears for an initial interview with a psychotherapist. This conversation takes place:

Patient: I am feeling very discouraged.

Psychotherapist: You are feeling very discouraged.

Patient: I don't feel like living any more.

Psychotherapist: You don't feel like living any more.

Patient: I am thinking of taking my life.

Psychotherapist: You are thinking of taking your life.

Patient: I feel like jumping out of the window.

Psychotherapist: You feel like jumping out of the window.

The patient jumps up, rushes over to the window, and hurls himself down to the street far below. The psychotherapist rises, walks to the window, looks down, waits and then says: "Plop".

Second, the idea of relieving distressing symptoms and restoring patients to health has a scientific flavor to it, but this should not conceal the evaluative character of the therapy. A patient may feel extremely guilty concerning his seduction of his seven-year-old boy, and the therapist will not decide that his main task is to relieve the guilt feelings.

IV. Moral Implications of Psychoanalysis

The question of the moral implications of psychoanalysis, in the restricted sense that I have adopted, presents some special problems. Freud says:

However much the analyst may be tempted to act as teacher, model, and ideal to other people and to make men in his own image, he should not forget that that is not his task in the analytic relationship, and indeed that he will be disloyal to his task if he allows himself to be led on by his inclinations. He will only be repeating one of the mistakes of the parents when they crushed their child's independence and he will only be replacing one kind of dependence by another.⁴

Some psychoanalysts, of whom Freud is one, seem to be confused about the relation between analysis and moral problems. Karl Menninger in his recent book says: "Neutrality in the analyst is one of the essentials of psychoanalytic treatment."⁵ But then he says: "He

will refrain from passing a moral judgment prematurely on what a patient mentions or fantasies or even contemplates doing.”⁶ If the patient might do something that would endanger his life or welfare or that of others, the analyst will “of course” express disapproval. This position is no more or no less neutral than that of a priest hearing confessions. Finally, Menninger closes his book on this rhapsodic note:

There is thus an implicit philosophy and ethic in the psychoanalytic experience, deny it who may. It is implicit that love is the greatest thing in the world. It is implicit that true love suffereth long and is kind, envieth not, is not puffed up, seeketh not its own (but the welfare of others), and rejoiceth not in iniquity but in the truth.⁷

If we then say that the psychoanalyst is not neutral, we can mean either of two things. First, we may mean that the psychoanalyst in his work is guided by the value orientation of the culture in which he lives. Second, we might mean that the psychoanalyst employs his own value orientation, which may be quite different from and inconsistent with that of the culture in which he lives.

What, more specifically, is meant by saying that the psychoanalyst in some ways employs a value system? The first circumstance under which he might do so is in his decision to accept or refuse a contract with the patient. There are a number of variables involved in considering this question: age of patient, financial resources, life situation, intelligence, presence or absence of psychotic components, etc. But the decision to accept implies that there are certain goals which a psychoanalyst believes can be achieved, and that these goals are worth achieving. It is convenient to divide this decision into a factual and evaluative pair. To say that psychoanalysis will achieve certain results is to make a factual claim. It makes no difference whether one says that A will produce B or that A might produce B or that A will not produce B, no difference, that is, as far as the purely factual nature of the claim is concerned. On the other hand, to say that B should be striven for or that B is worth achieving is clearly evaluative in character. If it is the case, as I believe, that the analyst makes evaluative judgments of this kind, then clearly he is not neutral in a scientific, disinterested sense.

Consider the case in which the candidate for analysis is a creative artist. Much nonsense has been written about the relation between neurosis and creativity. There are, however, at least three interpre-

tations of the relation. One, the artist is creative in spite of the neurosis. Two, the neurosis contributes to the artist's creativity. Three, there is no connection between the neurosis and the artist's creative powers. Let us suppose that in a given situation the psychoanalyst believes, as some have believed in particular cases, that the neurosis contributes to the creative performances, and that if the patient is cured, his creative powers well may wane. Is he to relieve the patient's distress and possibly deprive the world of his artistic masterpieces, or is he to sacrifice the person for his art? It is for reasons such as these that I do not believe the psychoanalyst can retain the mask of objective neutrality. I should hasten to say that some analysts insist that they are not entitled to wear such a mask.

V. Ethics and Psychodynamics

Let us now consider the evaluative presuppositions and implications of central concepts in psychodynamics, the theories surrounding both psychotherapy and the special form of psychotherapy called psychoanalysis. Some of the pertinent terms are 'adaptability', 'reality testing', 'healthy', 'unrealistic', and 'rigid'. Some of these terms have pejorative connotations, and it has been pointed out that in the development of psychodynamics, a number of words from ordinary language were taken and employed in new ways. When the layman and specialists from other fields encounter these words in a psychodynamic context, they may not divorce old meanings and feelings attached to these words from the ones newly stipulated. It is easy to say that we should not misinterpret the technical meanings of these words by sliding back into the conventional meanings. The question arises, however, as to whether psychotherapists use these terms in these technical contexts, devoid of their old associations, in an evaluatively neutral way.

Consider, for example, the word 'adjustment'. One can claim that this word is evaluatively neutral. According to the theory of organic evolution, the human organism cannot survive unless certain adjustments to reality are made. This fact *can* be noted without any commitment concerning the value of human survival. In practice, however, 'adjustment' in psychiatric contexts frequently means adjustment of the individual to the cultural environment in which he lives. The question of whether adjustment to the environment is appro-

priate in view of the individual's values and interests and whether this adjustment will produce intra-psychic conflicts is, comparatively speaking, de-emphasized. A given psychotherapist might challenge this statement and claim that he and others, in establishing goals for psychotherapy, consider seriously the individual's values and interests, and that at times the outcome of therapy is modification of the environment or transition to a new environment. Be that as it may, it is clear that in either case, the concept of 'adjustment' is value-laden.

The words 'flexible' and 'rigid' are used frequently by psychotherapists in describing human behavior. In a recent book, Dr. Kubie has attempted to give some reasons why it is inappropriate to say that the artist *qua* artist is ill. He claims that a measure of health is flexibility. "The essence of illness is a freezing of behavior into unalterable and insatiable patterns."⁸ Now the description of a person as flexible or inflexible can be value-free. Some people can easily adjust their demands and desires to environmental changes. Others rigidly maintain the same demands and desires even though environmental changes may have rendered them inappropriate. Even here, however, when is one being rigid and when firm? When is one being flexible and when spineless? And what are the criteria of appropriate and inappropriate actions? Consider the behavior of Graham Greene during a visit to Evelyn Waugh. Both men were supposed to be working, but Waugh discovered that Greene would disappear from the house for hours, and reports:

When I finally asked him what he was doing, he explained that he could not write another word until a certain combination of numbers . . . appeared to him by accident. He was spending all his time by the roadside waiting for those numbers to pass on a license plate. He could not write another line until he saw them. Well, it is a poor country road, and there are not many motorcars. Graham had a long wait.⁹

Most psychotherapists, I surmise, would regard this pattern of behavior as being rigid as contrasted to firm. Suppose, on the other hand, that a research scientist spends his entire life with the inalterable aim of finding a vaccine to control a given disease. Perhaps he never produces a vaccine or, if so, in the last days of a long life. It is at least doubtful that we would characterize his behavior as rigid and inflexible. Inflexibility, in many contexts, is regarded as

symptomatic of illness, and the job of the psychotherapist is to do what he can to reduce rigid behavior patterns and increase flexible patterns. I am suggesting, then, that in the practice of psychotherapy, words that *can* have an exclusively descriptive function are frequently employed in ways that reflect moral decisions. I suspect that the emphasis in our culture on adjustment of individuals to society is reflected in psychotherapeutic contexts.

VI. Therapy and Psychotherapy

One may wonder why there is so much interest these years in the moral implications and presuppositions of psychotherapy and psychodynamics, and relatively little attention is paid to the relations between morality and ethics, on the one hand, and the various forms of physical therapy, on the other. Surely, no one would deny that the surgeon, the internist, and the pediatrician presuppose that certain states of human organisms are better than other ones, and proceed accordingly. Nonetheless, there is much less dispute about what constitutes organic health than there is about what constitutes mental health. The average physical therapist accepts norms that are, relatively speaking, fairly clear cut with respect to organic health. A dramatic exception to this is euthanasia, but my point is that it is an exception.

The average psychotherapist, contrariwise, must participate more actively in determining what are the norms of mental health. Freud said that mental health is "The capacity to work and to love." Dr. Leon J. Saul in a book published last year modifies this to read: "the capacity to enjoy loving and being loved and also responsible working and harmless recreation, in proper balance."¹⁰ The vagueness of this slogan is such as to demand specific interpretation by the psychotherapist. In an attempt to explicate this goal of mental health, Dr. Saul says that the "therapist only frees the healing and developmental tendencies of Nature."¹¹ And he suggests that this can be learned from paradigm examples. Thus he points out: "Biographies of such men as Jefferson and Lincoln also reveal the nature of emotional maturity."¹² I believe that it was Dr. Brill who diagnosed Lincoln as schizophrenic. We need to have more robust criteria than Freud and Saul present, if we are going to be able to agree upon the models of maturity and health in our culture.

VII. Metaphysics and Psychoanalysis

The bridge between the ethical and metaphysical involvement of psychoanalysts can be found in Freud's treatment of religious people as fixated or regressed personalities. I noted earlier that the view of metaphysics that I am utilizing is one whereby it has the task of relating and correlating all specific systems of description and explanation. There are no limit-setting principles applied to the metaphysician; he may obtain his building stones from any quarry he pleases. Or so it was. Recently, one of the sciences that philosophers have frequently utilized in constructing metaphysical systems has attempted to explain metaphysics itself. This science is psychology, more specifically, psychodynamics. The program is announced by Freud in this way:

As a matter of fact, I believe that a large portion of the mythological conception of the world which reaches far into the most modern religions is *nothing but psychology projected into the outer world*. The dim perception (the endopsychic perception, as it were) of psychic factors and relations of the unconscious was taken as a model in the construction of a *transcendental reality*, which is destined to be changed again by science into *psychology of the unconscious*. It is difficult to express it in other terms; the analogy to paranoia must here come to our aid. We venture to explain in this way the myths of paradise and the fall of man, of God, of good and evil, of immortality, and the like — that is, to transform metaphysics into meta-psychology.¹³

The program for transforming metaphysics into meta-psychology, including the myths of good and evil, refers to the procedure of tracing projective material back to its origin in the unconscious. Let us examine in some detail the relationships between mythology, metaphysics, and meta-psychology. Once again I am operating primarily within the context of the works of Freud. But the convictions to be found there in these areas are shared by many contemporary psychoanalysts. A first defensive move for a metaphysician to employ against Freud's reduction of metaphysics would be to ask whether his statement is self-referring. Should Freud's metaphysical views, if such there be, be construed exclusively as projective data? All overt metaphysical statements would then have a self-defeating air to them. I wish to show, however, that some of Freud's metaphysical assertions are properly interpreted as objective claims. Perhaps these are

capable of a dynamic interpretation, but this is secondary to their primary philosophical interpretation. In other cases, however, one searches in vain for a significant philosophical interpretation of some of Freud's assertions, and here it may be that a psychoanalytic approach is indispensable. My contention is that we should neither attempt a wholesale reduction of metaphysics to meta-psychology nor declare that metaphysics is immune to meta-psychology.

Freud opposes the theory according to which science yields exclusively subjective results, the real nature of things outside us remaining inaccessible. Thus he says: "Scientific work is our only way to the knowledge of external reality."¹⁴ He mentions a number of arguments against subjectivism, but does not develop any one of them effectively. First, he argues that our mental apparatus has been developed in the attempt to explore the outer world, and therefore must have realized in its structure a certain measure of appropriateness.¹⁵ Whatever empirical meaning this argument has is a function of the meaning of 'appropriateness', and Freud does not explicate this concept. Second, the mind itself is part of the world, and hence admits of investigation by scientific procedures. One can guess here that Freud is rejecting psycho-physical dualism, but it is not easy to discern what evidence he has in mind. Third, "The problem of the nature of the world, irrespective of our perceptive mental apparatus, is an empty abstraction without practical interest."¹⁶ It is easy to see that Freud is not beating a dead horse. In a recent book, Kenneth Colby says:

... We never deal with environed reality *as it really exists* but with *intrapsychic representations* of it in the form of concept-meanings. We are accustomed to saying that by moving the head we scan our surroundings, but it would be more accurate to state that we are scanning percepts, i.e., psychic products related to, but not a completely accurate representative of, events in the environment ... Beyond the space-time world we are now familiar with lies some other kind of physical world which we have only limited chances of imagining.¹⁷

For Freud, Colby's unconceptualized objective reality is an empty abstraction. Of course, we can make specific alterations in our perceptual apparatus and produce alterations in perceptual experience. For example, we can squint our eyes or rub our eyeballs vigorously, but to ask what the world be if it were unperceived or what it is as unperceived is to paralyze inquiry. No one knows what would count as an appropriate answer. I suspect that Colby believes that physics

is best able to describe the world beyond our world; he does say that "... things are not as they seem in the material world ..."¹⁸ Physics does develop extraordinarily sophisticated maps, models, and theories, and these are highly effective for the sorts of descriptions and predictions that physicists make. But these are no more immaculate or ultimate than pronouncements from the market place or the poets' corner. The psychodynamically oriented psychiatrist is tempted to accept the concept of ultimate descriptions, using etiological priority as the criterion, but this can have dangerous consequences. The boy who loves his little sister so much that he hugs her intensely, bruising her, may unconsciously wish to harm or eliminate this rival for his parents' affection, but this does not nullify the fact that he does love her, i.e., he feels affectionate towards her, wants to give things to her, care for her, caress her, and cries and feels bad when he discovers that he has bruised her. (One reason for the vogue of existential psychiatry is the attention that it devotes to properties and relations that are observable here-and-now.) Whether it is more important to observe the boy's "loving" behavior or to investigate its origins is a function of the type and stage of inquiry. Within contextual frames, it makes good sense to speak of a basic or ultimate type of description; without a contextual frame, it makes no sense whatever.

Freud's empiricism, then seems to me to be vastly superior to Colby's transcendental assumption. One might wish to investigate psychoanalytically Freud's acceptance of empiricism, but my point is that one can talk about philosophical evidence for it without using a psychoanalytic frame of reference. Consider, on the other hand, Freud's concept of a death instinct. As Jones points out,¹⁹ Freud places his theory of a death instinct in a metaphysical setting whereby the distinction between *telos* and *finis* is eliminated. If someone commits suicide – and think of a clear-cut case in which a person is unhappy, announces that he will kill himself, and then does so – it surely is appropriate to say that he aimed at his own death. If someone accidentally takes poison, it *may* be appropriate to say that he unconsciously intended to kill himself. Whether it is appropriate or not can be determined only by evidence in the specific case. Are we to say, however, that anyone who dies from any cause you please *aims* at his death? The oak tree is struck by lightning and dies. The bean crop is killed by flooding waters. The granite sculpture erodes away, and the waves beat upon the shore. Perhaps all activity is aimed at self-destruction.

What can be said in support of Freud's theory of a death instinct pervading nature? Freud's associates could not produce evidence in its support, and philosophers find it to be useless as an organizing concept. Jones says:

If so little objective support is to be found for Freud's culminating theory of a death instinct, one is bound to consider the possibility of subjective contributions to its inception, doubtless in connection with the theme of death itself.²⁰

Jones then points out that Freud, even in his prime, hated the thought of growing old and said he thought of death every day of his life. Jones concludes:

In the light of all these considerations I think it fair to suggest that in forming an opinion about the validity of Freud's theory of a death instinct we are justified in taking into account possible subjective sources, in addition to the arguments he adduced in his writings.²¹

Note that Jones suggests a psychoanalytic interpretation of the death instinct theory after other interpretations had failed. That is, he was unable to find any other plausible interpretation. Frequently, I suggest, psychoanalytical interpretations of philosophical theories are most helpful when other interpretations fail.

Freud's linkage of philosophy, metaphysics and ethics in particular, to mythology is a case of attempted guilt by association. I should think that he was using the word in the way described by the Oxford English Dictionary:

A purely fictitious narrative usually involving supernatural persons, actions, or events, and embodying some popular idea concerning natural or historical phenomena. Often used vaguely to include any narrative having fictitious elements.

According to S. H. Hooke, a myth with its associated ritual is something which meets a recurrent human need, and we can safely say that this need is for life and prosperity in one form or another.²² I think it is futile to attempt to arrive at a standard unequivocal and encompassing use of the word 'myth'. The word is multifunctional, and the range of phenomena referred to by various uses of the word is unfixed. I have discovered that some people use this word as a proper name, i.e., they are taught to apply the word to the story of

Medusa and/or Santa Claus, but are unable to provide any criteria of application. At times, a myth might be a primitive, fumbling attempt to explain the world. It may be superstitious, and acceptance of it may be a symptom of immaturity. Nonetheless, mythology is not inherently or inevitably a disease and, in some cases, it fulfills positive functions. Field anthropologists describe the ceremonies of various tribes that are believed to cure sickness, stop famines, etc. As Joseph Campbell points out, they also describe ceremonies which imply a pre-supposed acceptance of nature and fate. Seasonal rites do not attempt to stop the appearance of winter, but prepare the tribe for winter. Other rites concern crops, not that they order nature to produce crops at any given time, but instead dedicate the members of the tribe to long, hard work. Many mythological legends are inappropriately referred to as primitive or immature, since they are based upon a firm grasp of reality testing. That is why Campbell maintains that one of the primary functions of myth is "to conduct people across those difficult thresholds of transformation that demand a change in the patterns not only of conscious but also of unconscious life."²³

I am inclined to think, then, that a myth can be extremely profound. It may, of course, be symptomatic of unconscious forces. To say this, however, does not commit one to a reductive procedure. It is compatible with an interpretation of myth whereby myth serves adult, mature functions. Perhaps metaphysics is properly interpreted as being mythological philosophy, and perhaps, as so interpreted, metaphysics is profound. I am not under the illusion, however, that I have shown this, and I am not even sure that I would know how to proceed.

NOTES

¹ Ernest Jones, *The Life and Work of Sigmund Freud*, NY, 1953.

² Sigmund Freud, *New Introductory Lectures*, pp. 202-3, 232.

³ *Psychoanalysis, Scientific Method, and Philosophy*, Sidney Hook (ed.), Washington Square Press, NY, 1958, p. 177.

⁴ Sigmund Freud, *An Outline of Psychoanalysis*, NY, 1949, p. 67.

⁵ Karl Menninger, *Theory of Psychoanalytic Technique*, p. 93.

⁶ Ibid.

⁷ Ibid, p. 179.

⁸ Kubie, *Neurotic Distortion of Creative Process*, footnote, p. 21.

⁹ Howard R. F. Sheehan, "Evelyn Waugh Runs a Fair", Harper's, January 1960, p. 33.

- ¹⁰ Leon J. Saul, *Technique and Practices of Psychoanalysis*, Lippincott, NY, 1958, p. 8.
- ¹¹ Ibid, p. 12.
- ¹² Ibid.
- ¹³ Sigmund Freud, *The Psychopathology of Everyday Life*, 1914, pp. 309–10.
- ¹⁴ Sigmund Freud, *The Future of an Illusion*, London, 1928, p. 55.
- ¹⁵ Ibid, p. 97.
- ¹⁶ Ibid, pp. 97–98.
- ¹⁷ Kenneth Colby, *Energy and Structure in Psychoanalysis*, NY, 1955, p. 129.
- ¹⁸ Ibid, p. 9.
- ¹⁹ Ernest Jones, *The Life and Work of Sigmund Freud*, III, p. 272.
- ²⁰ Ibid, p. 278.
- ²¹ Ibid, p. 280.
- ²² *Myth, a Symposium*, Thomas A. Sebeok (ed.), Indiana University Press, 1958, p. 77.
- ²³ Joseph Campbell, *The Hero with a Thousand Faces*, p. 10.

PSYCHOANALYSIS, MAN, AND VALUE

by

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Psychology does not seek to correlate independent happenings, but to discern in the manifold of a unique life, or of a unique culture, the pervasive sense or meaning which the life or culture expresses. The method is appropriate to the subject: a human being is not a string of lawfully connected events but an embodied meaning, an incarnated value. Psychology is therefore less like physics than it is like the critical interpretation of a work of art. It is psychoanalytical. Some of the traditional objections to psychoanalytic theory disappear in the light of this conception. Others vanish when it is realized that the analytic situation, being personal encounter as well as interpretative analysis, to some extent creates the being it analyzes.

In the broad sense that we come to understand them through familiarity with what they do and say, our knowledge of people is undoubtedly empirical.

The term empirical is often, however, specified more narrowly. In this narrower sense we understand a happening empirically only if we know of a dependable correlation between happenings of that sort and happenings of some essentially independent sort. The empirical method so conceived I shall call the inductive method. It is the method often attributed to physics and often, indeed, simply identified with the scientific method. It is the method which many social scientists are seeking to use in the study of man.

As has often been remarked, psychoanalytic theories fail in various ways to measure up to the demands of the inductive method.

The behaviorist notes that what a person does cannot be correlated with his inner state, since his inner state is in principle unobservable. An inductive science of man must therefore restrict itself to correlating types of behavior. But the kind of behavior which the psychoanalyst would like to study is so subtle as to be beyond the available techniques of behavioral science. In the present and foreseeable future, therefore, psychology must be content to investigate more simple behavior: twitches, blinks, and the learning and forgetting of nonsense syllables. What this argument amounts to is the claim that there is no such thing

as indirect verification, that all verification must consist in direct observation of the sorts of events in question. It is doubtful whether even physics could live up to such a radically operationist demand. Nonetheless, it must be granted that if the inductive method does not entail operationism and therefore behaviorism, it is difficult to see why it does not.

Even if, overlooking the behaviorist argument, we allow the possibility in principle of correlating behavior with inner states, we are simply brought face to face with the experimentalist charge that analysis has conspicuously failed to demonstrate that any particular item of behavior is indicative of any particular inner state. There are no standard dream-symbols, standard symptoms, or standard test-responses. No test, no dream, no symptom can be interpreted "blind", in isolation from other data. Analysts claim that each item is significant if interpreted "in the light of our other knowledge", as they say. But if no one item means anything, how can a number of them together do so? If each item in isolation gives a probability of zero, then the product of them is also zero, and the analyst should speak, not of interpreting the present item in the light of his further knowledge, but of interpreting it in the light of his preconceived theories.

Thirdly, even a sympathizer must admit that analytic theories which generalize beyond a single individual turn out to be wrong. Certain statistical correlations may indeed be generalizable, and many social scientists profess to be content with that. A statistical law is not, however, knowledge at all, but, as Plato said, right opinion. That sixty per cent of Irish, urban women with working mothers and one older brother vote Democratic or become alcoholics does not tell us why anyone votes Democratic or becomes alcoholic. An inductive science inevitably seeks causal laws, simply because it wants to find out "why". And of a causal law, or so anyway it is widely held, universal conjunction is at least an essential part.¹

Such considerations force me to the conclusion that analysis not only has not, but cannot, meet the demands of the inductive method. If, therefore, the inductive method were the appropriate method of studying human beings, it would follow that analytic theory is not knowledge at all, but sheer speculation. That, however, is a conclusion which no one with any first-hand experience of analysis can admit. The knowledge gained through psychoanalysis is not complete. It is not incorrigible. But it is knowledge.

Not that it is self-evident. I have already said that it is, like an

hypothesis, based indirectly and inferentially upon observable facts. Unfortunately, the facts upon which it is based are not easily observable outside the privacy, the confidence and the urgency of the analytic situation itself. And, as revealed within the analytic situation, they are so subtle — everything depending upon a momentary hesitation, a slight awkwardness, a change in respiratory rate — that they can be only inadequately described to a third person. Even the longest case history fails to produce the same conviction as a first-hand observation of the facts. To a large extent, therefore, the data upon which psychoanalytic knowledge is based are private to two people. And it is true that neither of these people could qualify as a completely unbiased observer. This should not, however, lead us to jump to the extreme conclusion that all analysts and all patients are completely blind. And if the privacy of the data underlying psychoanalytic knowledge be thought a crucial objection, then three things should be said. First, the privacy is not absolute. Motion pictures, case histories, and patients' reports can do something toward making the relevant facts public. Secondly, very much of our knowledge rests upon data which are available to one person only — my knowledge that I have a headache, for example. Or, if that be thought an incomparable case, then consider the understanding I have of my wife. Obviously I know a great deal about her. But I should be hard put to justify my knowledge to someone who has not lived with her. Thirdly, the data are available to any qualified individual who becomes an analyst, or a patient, so in that respect psychoanalytic data are hardly more private than the data of the physicist.

The sorts of reasons I can give in support of my notions about another person are not of course exactly the same sorts as I can give when what is in question is the principle of the reciprocating engine. But then, I am not wholly without reasons either. In ordinary life, you and I often disagree about our mutual acquaintances only to find that, after each has given his reasons, the other of us is convinced. Both phenomenology and Oxford philosophy have made us aware, in recent years, that "clear," "sharp" distinctions between analytic and synthetic, fact and value, belief and attitude, freedom and determination, body and mind, are likely to be too clear and sharp to be tenable. I hope we can apply the lesson to those who cry "Mere intuition, mere subjective conviction, a mere hunch!" whenever we are unable to support our claims to knowledge with one particular sort of reasons which they assume to be the only real *reasons*.

But if human behavior were, like that of inanimate things, determined by causes, then the method of physics would be the appropriate way of understanding it. We cannot reject the methodological assumption without discarding also the substantive assumption upon which it is based — that people are causal mechanisms. Perhaps the chief lesson psychoanalysis has for philosophy is that it indicates that human beings are not causal mechanisms² and that it gives us a clue to a quite different notion of what they are.

In discarding the universal determinism on which Freud prided himself above all and which he regarded as an absolutely necessary presupposition of psychoanalysis, I am not suggesting that we reinstate the idea he fought so hard against — the idea that human life is a loose aggregate of disconnected, unmotivated, meaningless acts. That would indeed make any science of man impossible. Freud was right, I think, that when we reach the point of conscious choice, already *les jeux sont faits*. There is a reason behind a life, a reason which, by manifesting itself in even the most trivial incidents, binds the life together, so that what one did at the age of four is not irrelevant to what one does today. The reason is even, in a sense, irresistible: we generally find to our chagrin that our New Year's resolutions manifest yet once again the pattern of the past. The reason behind a person's actions can be changed only by rebirth. And that, as is known, is a matter of grace, not of works. One legal entity may of course live more than one life, either concurrently, as in dissociation, or successively, as in conversion. Indeed, I suspect that each of us lives, successively, a plurality of lives. Every really fundamental choice which we make does not so much redirect our life as commence a new life. That is why, in the case of a fundamental choice, we can never find any reason for one decision rather than another. And why, the choice once made, it becomes meaningless to ask whether it would have been better to have decided otherwise. To ask this is as nonsensical as to ask whether John's decision to study philosophy is better or worse than Robert's choice of history. And for the same reason. But although one legal entity may lead more than one life, each life is stamped out of one mold by the reason which dominates it. In that sense, the reason is irresistible. Only, this reason is not a cause.

What I propose to substitute for determinism is an idea of which Freud made almost as much: the idea of universal meaningfulness, the idea that man is a symbolizing animal who gives a meaning to all that befalls him. Freud's view was, to be sure, that the meanings a

person embodies in his experience are products of prior causes, so that determinism is more fundamental than symbolism. What I propose is the reverse of this: that a person's past is a cause of his present action only in so far as he endows it with meaning, thereby finding in it a motive for present action. We may, in a loose sense, speak of motives as causes, but they are quite unlike those in inanimate nature. For they are already endowed with the same meaning as their effects, and it is only as so meaningful that they are causes. Nor is the bestowal of meaning on the cause even temporally prior to the bestowal of meaning on the effect. Indeed, there are not two successive bestowals of meaning at all, but a single *Gestaltung* of the whole life, past, present and future. To illustrate the difference between cause and motive, let us recall Freud's discovery that in the course of treatment hysterical women often recovered memories of childhood seductions. This fact became important in his understanding of their cases. Later, he was upset to find that in many cases no such seduction had occurred. Given his deterministic assumption, he had reason to be upset: what has not happened cannot be a cause in the sense of the physical sciences. But a false memory is as good a motive as a true one. For it has the same meaning.

The reason which dominates a person's life is neither a cause nor a motive, for it is not an event at all. It is exactly the meaning which all events in a life share. Indeed, it is just the person himself. A person does not give meaning to his life, he *is* the meaning of his life, the peculiar *Gestalt* which informs the many happenings of his life.

By the meaningfulness of a life, I do not mean primarily whether the life is found good or bad, purposeful or senseless. Generically, the meaning of a life consists simply of the significance which the "bare facts" take on when they are incorporated into that life. It consists of what a person reads in the faces of things and of other people: a threat perhaps, a promise, an invitation, an enigma, an absurdity. It consists in finding that the object in one's hand is either a glass of water, a fire-extinguisher, a weapon, a paper-weight, or an artistic failure.

Since, in the human realm, causality presupposes the meaningfulness of the cause, it would be circular to explicate this meaningfulness in causal terms. The meaning is not something external to the symbol, but causally connected with it, like clouds and rain. A person is not external to his life. What I do is not an effect I produce but a symptom of who I am. And just as a symptom is not an effect of a disease, but an expression of it, so my life expresses the meaning that I am. Nor is

the meaning of a life really behind the life, hidden, like the meaning of a word in an unknown language. It is in the life, like the meaning of one's native tongue.³ Conversely, a person's body is not, as Descartes thought, external to the pure consciousness which is what he is, an *object* of that consciousness. The body is an expression of the person, as inconceivable without the meaning it expresses as is the person without his body.

If you begin by separating the inner from the outer, the person from his body, then of course you find that the inner is out of reach and you are likely to end a behaviorist. But if the person is a meaning which is not behind his life but in it, then there is no need to get at the person by correlating what can be observed with what cannot be. Rather, one "interprets" the life, one "reads" its meaning. If we had to get at the meaning of speech by correlating words and syntactical structures with their already familiar meanings, we could never be told anything we did not already know. Language can communicate only because we can read the meaning in the language itself. Similarly, a person communicates himself in everything he does. "He who has ears, let him hear." The qualifications of a psychologist are not experimental diligence, statistical ingenuity, or even abstract clarity. They are the talents of a good reader: sensitivity, discernment, sympathy, imagination, an ear for nuance. Without ceasing to be a science, psychology is also a humanity, for it is the science of what is human.

That the meaning is in the life does not imply that we can read the meaning from an isolated segment of the text, any more than the meaningfulness of a piece of music implies that we can discern the meaning from listening to bars 101 through 105 alone or any more than the meaningfulness of a language implies that we can know what "an" means without waiting to see whether it is followed by "al", by "alyst", or by a blank space. It is, I suggest, the same with human action. Each act has a meaning, but we can understand that meaning only "in the light of our further knowledge". This is because a single action is essentially syncategorematic, essentially a part of a larger context, like the first part of taking a step or of rising from a chair.

There are, to be sure, relatively autonomous circuits within a person which are close to the chemical or electrical level: such things as reflexes, sensation, and metabolism. On this level we may speak of "behavior", "sense-data", "pleasures" and "pains". And to the extent that these circuits approach the status of causal mechanisms, the inductive method is an approximately appropriate way of studying them.

But even these elementary functions are not impervious to their integration within a larger psychosomatic whole. Human "behavior" has a meaning, and so is not behavior, but action. Human pleasures and pains have a meaning and so are not pleasures and pains but happiness and misery. Human sense-data are incorporated into the *Lebenswelt* of significant things. And this whole, which transfigures even the simplest functions, is not a causal mechanism. It is, like a work of art, an expression of a meaning. And, as in a work of art, meaning and expression are inseparable and yet irreducible. There is no human experience which is not meaningful, no meaning not incarnated in a life.⁴ It is because a person is like a work of art that he is to be understood, not through correlations, but through critical interpretation — or, as we may say, through analysis.

Two lives may share certain strands of meaning in common. When they do, one interpretation will be true of both. In that sense, an analytic interpretation may be general. This generality is not, however, something which can be attained through generalization. For how am I to know whether two lives share a common meaning except by analyzing each in its own terms? I could generalize from the one to the other only if there were certain standard symptoms which always had the same meaning. And that is exactly what there is not. Two lives may agree in such externals, such "bare facts", as being Irish or urban or having working mothers or older brothers and yet have entirely different meanings. In one, the mother is neglectful, in the other, successful; in one, the brother is privileged, in the other, nurturant; the one is Irish in the low-comedy-sense, the other in the St. Patrick's-Day-sense. In a human life, the "bare facts" are of no importance. The facts of human life are of account only because they are already dredged in a meaning. And what meaning a life is dredged in can be discerned only from that life.

An analyst may also seek generality through taking as his goal the analysis, not of an individual life, but of a culture. Now, however, it is the life of that single culture which he must interpret. And into that cultural life the lives of individuals fit, not as instances of a universal which exists entire in each, but as partial phases, parts of a whole, like the various themes of a sonata or the characters of a novel. Our understanding of a culture no more permits us to generalize about the individuals who make it up than our understanding of a novel permits us to infer that the last chapter must have the same meaning as the first.

Whether the subject be an individual or a culture, therefore, analysis is stuck with the unique. If a human life were a watch, to be understood by the inductive method, this fact would be fatal. But a life is not a watch. It is, like a poem, the expression of a meaning. And the interpretation of a poem need not be, indeed could not be, generalizable. The interpretation of a poem requires no essential reference beyond this unique poem. It is neither necessary nor useful to argue that because a certain expression or image has meant such-and-such in one poem it probably means the same thing in another. Instead of analyzing one poem, one might, of course, analyze Shakespeare's poetry, or English poetry. But then Shakespeare, or England, is the unique individual. In poetry there are no standard, generalizable symbols. Similarly, in psychoanalysis, there are no standard dream-symbols. Yet dreams do mean something. And just as a critic may understand a poem better than its author, so an analyst may understand a dream better than its dreamer. Finding oneself stuck with the unique is a defeat of understanding if what one is studying is a causal mechanism, but not if it is a language, a work of art, or a person.

The objections to psychoanalysis which I stated earlier would be fatal if analysis purported to be an inductive science. But if analysis is not correlation, but interpretation, they are without force. All of them rest upon the notion that to understand a happening is to know a universal correlation between happenings of that sort and happenings of an essentially independent sort. All of them vanish when that conception of understanding is overcome.

The behaviorist, on the assumption that any science is concerned with essentially independent happenings, argues that, since inner happenings are in principle unobservable, psychology must study behavioral happenings exclusively. That which the analyst seeks is not, however, an inner happening. It is a person. And a person is not independent of, external to, his observable behavior, but is in his behavior, as its meaning.

On the same assumption — that what is in question is happenings — the experimentalist argues that each behavioral happening, taken by itself, ought to be correlative to some inner happening. He then objects that psychoanalysis has failed to find such correlations. But it does not follow that because each word has a meaning therefore each syllable does, or that because the painting has a meaning therefore each brush-stroke does. One must find the meaningful unit.

The third objection rests upon the demand, essential to the inductive

method, that the two classes of happenings to be correlated be essentially independent. If that were the case, the behavioral happenings could not essentially involve the subjective meanings with which they are to be correlated. From that it would follow, in turn, that the behavioral facts are "bare" facts, meaningless facts. Hence, since the same bare facts appear in many lives, the psychoanalytic interpretation of these facts in one life, or in a sample of lives, ought to be generalizable. In fact, however, analytic interpretations are not generalizable. This is because, in a human life, the bare facts are unimportant. What is important is the meaningful fact. And while the same bare facts may indeed appear in many lives, that is no indication that these lives have the same meaning. It is difficult, in fact, to get a sample that contains more than a single case. If, in the attempt to broaden somewhat the inductive basis of your interpretation, you increase your sample to twelve or twenty intensively studied cases, you are likely to find, as a result of your intensive study, that actually each case is unique. You are likely to find that the common characteristics on the basis of which you selected your sample (age, sex, social class, whatnot) are merely externals which have a quite different meaning to each individual. It is of course possible that the same "bare facts" may have the same meaning in more than one life. But whether this is so cannot be known by generalizing from any selected number of cases. It can be known only by discovering the meaning which those facts have in each life.

The analytic method contains analogues of all of the essential features of the inductive method. The meaning which is expressed in a life is analogous to a law. The correlation of behavior with motives is analogous to the correlation of events with causes. Considering the larger meaningful unit of behavior is analogous to multiplying probabilities. The finding of a meaning which is common to many lives or of a cultural meaning which includes the meanings of individual lives as parts are both analogous to generalization. Analysis even has an analogue of prediction, for, while it cannot predict what a person will actually do, it can and does predict what will be the meaning to him of whatever he does. But all of these terms are here used in a very extended, a merely analogous, sense. For all of them come to just this: the discerning of a single meaning which pervades an entire life, as found in the past as well as predicted of the future, and which may be partially shared by many lives or be part of the meaning of a culture.

But if psychoanalysis yields genuine knowledge, albeit knowledge

based upon interpretation rather than "induction", why is it that incompatible diagnoses seem to be equally successful in practice? Does this not indicate that therapy works, not because it is based on true knowledge, but for the same reasons that persons in authority, if they are wise, have always been able to help others: because confession is good for the soul, because suggestion is a power, because a friend in need is a friend indeed? Is not psychoanalytic theory just an irrelevant appendage of therapy, a mumbo-jumbo which conceals the fact that therapy consists of nothing but the old-fashioned home remedies — catharsis, suggestion, and encouragement?

What can be said in reply to this argument? First, this: that in some cases, to some extent, analytic theory is irrelevant to practice — that to some degree therapy is a matter of support, encouragement and suggestion. Secondly, that much of the disagreement among psychoanalytic theories concerns the etiology of a personality structure. Concerning the diagnosis of that structure there is less disagreement. Thirdly, and philosophically by far the most important, that even where there is basic disagreement between two interpretations of what a life means, and where both interpretations are successful in practice, this does not imply that the interpretation is irrelevant to the practice. It may be, on the contrary, that both analyses are successful because both, although they are in a sense incompatible, are nonetheless true.

There is no one true interpretation of Sophocles' *Electra*. That does not imply that no interpretation is true or that all are. What it implies is that *Electra* is not a thing-in-itself which pre-exists somebody's appreciation of it. Thus what seem to be incompatible theories of *Electra* may not really be so, for they may have to do with *Electra* as it is for and within different encounters.

Understanding a person is not, of course, exactly parallel to understanding *Electra*. A human being, unlike *Electra*, has an existence for himself. And the analyst is not seeking to grasp what the patient's life means to him — to the analyst, that is — but what it means to the patient. In this respect, analysis is something like trying to understand what *Electra* meant to Sophocles. Or, better yet, it is like trying to understand what one of Rembrandt's self-portraits meant to Rembrandt.

But what a human being is to himself is not something fixed and stable which remains unchanged through a succession of encounters. Nor should we make the mistake of thinking of it as a kind of essentially independent thing-in-itself, something which can indeed be "in-

fluenced" by its encounters, but is not essentially constituted by them. For to some extent a human being exists for himself only in and through the existence he has for others. One moment of what he is for himself is what he is for those who encounter him. Since analysis is a form of encounter, and a crucial one at that, is understandable that an analyst to some extent brings into being the very being he understands. This is not, as is often suggested, because of the illegitimate interference of suggestion. It is not, therefore, something an analyst could avoid with greater care. It results from the nature of human reality, from the fact that a human being incorporates his mirror-image. The beloved really is perfect, but only when the lover sees her so. And to understand *Electra* is, as Croce says, to create it.

From this it follows that, where two different theories are the outcome of genuine encounters with a patient, they need not be incompatible. For the patient is not wholly the same person within the one encounter as he is within the other. For the same reason, one cannot really say that two analysts are equally successful in practice since, strictly, two doctors never treat the same patient. And it would be odd indeed if the patient were the same in *any* analytic encounter as he is in a testing situation or as he is in his daily life. In the end, therefore, perhaps the best analogy is not to Sophocles' *Electra* but to a number of men — Aeschylus, Sophocles, Euripides, O'Neill, Sartre, and so on — each setting about to write a play on the life of *Electra*. Not all of their plays need be true, but more than one of them may be. For to some extent the subject of their plays, *Electra*, exists only in and by means of their plays about her.⁵

It is generally recognized that values of various sorts surround and sometimes intrude into the analytic situation. But usually these values are assumed to enter the picture only externally and tangentially, not as something which occupies the very center of the picture and without which psychoanalysis is inconceivable. The patient's values are among his symptoms, but they do not constitute either the illness or its cure. The analyst's values determine his selection of patients, his fees, his professional ethics. But they are not integrally involved in his understanding of the patient, since this is supposed to be purely cognitive, objective, value-free. And if the analyst's values do sometimes influence his treatment, manifesting themselves in such advice as he gives or in his judgment of what constitutes a satisfactory cure, this is taken to be at best a necessary evil.

But there seem to be at least three ways in which values are not just

stubborn obstacles in the path of therapy, not something irrelevant to analytic understanding, but essentially ingredient in both the truth and the practice of analysis.

First, the meaning the world has to a person consists in the values and disvalues he finds in it. In trying to discover a person, therefore, the analyst is trying to discover the values which constitute that person.

Secondly, if an analyst is to be a sympathetic reader of my poem, he must not read with the aloof, suspicious attitude of the Other, as if he were a visiting tourist. If he does, he will find me only a native. To understand me, he must come to see the same faces of things that I see. He must, that is, come to share my values.

Thirdly, however, the analyst must not really share my values. For to see the world through the values of another whom one encounters is to surrender oneself to him. It is a form of love. The analyst must, to understand me, come to the brink of loving me. But in the end he must draw back, he must resist the "counter-transference", he must remain an Other. He must do so for my sake, for I did not come to him for love, but for analysis. He must do it also for his own sake. For analysis is a personal encounter. And while it is not usually as crucial for the analyst as for the patient, it is not something he sloughs off without remainder at the end of fifty minutes either, as the chemist removes his white coat at five o'clock and goes home unchanged to his wife and children. The analyst must preserve his own being in the face of an encounter which threatens it and in spite of the patient's subtle attempts to possess him. But to encounter a person without in some sense loving him is to fit him into one's own world, to judge him, to measure him against oneself, that is, against one's own values. Nor should the fact that the analyst of necessity evaluates the patient be thought an obstacle to the only kind of objectivity which is possible in the study of people. For, as I have said, part of what the patient is to himself is what he is for those who evaluate him.

NOTES

- ¹ This third objection admittedly raises difficult questions which are by no means adequately dealt with in these few dogmatic sentences. If the claim that an inductive science seeks universal correlations were not granted, I should have to rephrase what I want to say in the sequel so as to admit that psychoanalysis, since it has discovered some statistical generalizations, has in this respect met, to a limited extent, the demands of the inductive method. But the fundamental

point I wish to make would not be affected. For it is obvious that an analyst knows a great deal more about an individual patient, and knows it with a much greater certainty, than he could justify on the basis of verified statistical correlations. So that even if we were to grant that the finding of statistical laws constitutes some sort of understanding, it would still be the case that it is in some other sense that the analyst understands the individual case.

² Actually, this conclusion could be extended to living organisms generally, but that is a further question.

³ Not that I think insisting upon the word "in" really explicates the symbol-meaning relation. In understanding human beings, the symbol-meaning relation is a primitive notion. The symbol expresses the meaning; the meaning is the meaning of the symbol. That is as clear as it can be made. To say that the meaning is "in" the symbol or "informs" it, or that the symbol "incarnates" the meaning: these are metaphors.

⁴ Perhaps it is going too far to *identify* this incarnated meaning with the person, as I did earlier. Perhaps there is a core to a person which is not an embodied meaning but something wholly inner, wholly inexpressible, a "ghost in the (meaningful) machine". But if so, this inward something is inevitably private and unknowable. It is the "me" which is *never* understood. The most I can do is to complain, like the eternal husband, that it is not understood; I cannot succeed in communicating it. And I can have only an abstract and general knowledge, based on analogy, that other persons also have this incommunicable, private center. In any event, even if there is such a center of inwardness, it is irrelevant to the present subject, since it is as unavailable to the analyst as to anyone else.

⁵ In other words, we have to do here with a phenomenon which is, as some philosophers prefer to say, "objectively relative". If "x is red" is elliptical for "x is red from such-and-such a place", then there is no necessary incompatibility between "x is red" and "x is not red".

ON THE SIGNIFICANCE OF HANNAH ARENDT'S *THE HUMAN CONDITION* FOR SOCIOLOGY

by

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Arendt's book is an analysis of the *vita activa*, which comprises the three human activities of labor, work, and action. Her presentation involves a critique of modern and current conceptions of them and of many other social phenomena, and an emphasis on distinctions customarily neglected. The interpretation of her book, disregarding the many factual statements it contains, proceeds in a theoretical vein, analyzing her major conceptions, and then turns practical, asking what we as social scientists who listen to her must do (focusing on "behavior" and "action", "values", the means-end scheme, and man's historicity and dualism). The paper concludes with a brief explication of areas of research seen to emerge out of Arendt's work.

One: Expository

The original title of Hannah Arendt's book, *The Human Condition*,¹ was to be *Vita Activa*.² It is an analysis of the *vita activa*, of its vicissitudes in Western history, and therewith of much of the story of Western man.

I

Arendt begins, however, with a systematic, rather than historical, description of the *vita activa*—the term she chooses to refer to the three human activities of labor, work, and action, each of which "corresponds to one of the basic conditions" (7) of human existence.

Labor corresponds to life itself. It is the activity man must engage in if he would live on this earth, where he is neither immortal, but is born and dies, nor self-sufficient, but must feed, clothe, and house himself. In laboring, the human body "concentrates upon nothing but its own being alive, and remains imprisoned in its metabolism with nature" (115; cf. 209) or "matter" (183, n. 8).

Work, on the other hand — a term which Arendt uses synonymously with "making" and "fabricating"—corresponds to man's

“worldliness”. It is the activity of producing the “human artifice”, that is, all the things that are used, enjoyed, revered, or contemplated. They are not consumed, however, for to be consumed is, precisely, the fate of the products of labor, not of work. And within the “artificial world of things” provided by work, “each individual life is housed, while this world itself is meant to outlast and transcend” (7) the life of the individual.

The third human condition, plurality (that is, simply the fact that man is not one but many), is the condition of action — “the only activity that goes on directly between men without the intermediary of things or matter” (7) — hence of all political life. “Plurality” must not be confused with “society”, which is not a human condition but only one of the historical forms of “plurality”.

II

To understand this distinction between “plurality” and “society”, and to understand the rise of “society” itself, we must learn about Arendt’s distinction between the Public and Private Realms. It is the distinction between the *polis* and the family-household, made in “all ancient political thought” (28), where the *polis* was the (public) realm of freedom, to which the mastery of the necessities of life in the household entitled one, while the household was the (private) realm of necessity, with the man laboring for individual maintenance, and woman laboring for the maintenance of the species.

The rise of society (from now on spelled without quotation marks) signifies the rise of economic activities — of private activities characteristic of the household — “to the public realm” (33). Society is essentially a phenomenon of the “modern age”, which began “in the seventeenth century [and] came to an end at the beginning of the twentieth century” (in contrast to “the modern world, in which we live today”, and which was born “with the first atomic explosion” [6]).

Society replaces action by behavior, and it equates the individual with his social status (which determines his behavior) — rank in the 18th-century half-feudal society, title in the 19th-century class society, “or mere function in the mass society of today” (41). In the earlier stages of society, its science was economics, which introduced and developed statistics to describe behavior — but only parts of the behavior of parts of the population. This germinal, though modest,

beginning was to develop "into the all-comprehensive pretension of the social sciences which, as 'behavioral sciences', aim to reduce man as a whole, in all his activities, to the level of a conditioned and behaving animal" (45).

Members of modern societies are, indeed, laborers or jobholders, that is, people who "consider whatever they do primarily as a way to sustain their own lives and those of their families" (46). And the society of jobholders, "the last stage of the laboring society" (322), no longer belongs in the modern (319), but in a later age (cf. 6).

As behavior and labor go with society, so does the division of labor, that is, "the organization of laboring", which presupposes (as image perhaps even more than as far as every individual's activity is concerned) a gigantic household wherein all members are harnessed to one or another of myriad mechanized labor processes. This mechanization of labor has gone so far as to allow us to forget "the verbal significance of the word [labor], which always had been connected with hardly bearable 'toil and trouble'" (48).³

"Public" refers, first (as an adjective), to that which "can be seen and heard by everybody" (50); second (as a noun), to the man-made world common to men, including the affairs that go on among them; and this world contrasts with "our privately owned place in it" (52). By virtue of this common man-made world, men are related and separated; by virtue of it, they *can* relate to one another and distinguish themselves from one another. Such a public, as against private, space "cannot be erected for one generation and planned for the living only; it must transcend the life-span of mortal man" (55).

Historically, Arendt claims, the most conspicuous proof and exemplification of the private realm has been property, which had the same obscurity — as contrasted with publicity — as had the other essential features of the household (the laboring of the man and the laboring of the woman). We are apt to forget this because of the modern identification of property with wealth. Wealth, however, is the individual's "share in the annual income of the society as a whole", while property, originally, meant "to have one's location in a particular part of the world and therefore to belong to the body politic" (61). The interior of the household, as well as birth and death, remains hidden from the public, but its exterior appears as the boundary which separates one household from the next; and the original meaning of the law of the *polis* was the boundary line between the private and the public (63–64 and notes 63, 65).

Thus, in addition to labor (and the division of labor) and behavior, a third characteristic of modern society is wealth and propertylessness. The simultaneous rise of wealth and propertylessness has meant the extinction of the difference between the public and private realms, hence their simultaneous disappearance in the social and their replacement by it. The modern age has emancipated both women and laborers, the two types of man who had not been part of public life but had been private, "laborers" in the two senses of the term; and "the few remnants of strict privacy even in our own civilization relate to 'necessities' in the original sense of being necessitated by having a body" (73).

III

The products of labor are destined to be consumed; they are consumer goods. Hence to speak of our society as one of consumers or one of laborers is merely to single out one or the other phase of the same process. The "thing character" of our world is guaranteed by "the work of our hands" the main products of which are use objects; and without use objects, we might not even know what a thing is (cf. 94). Labor produces no things. It is a process, repetitive, self-perpetuating, fertile, which has emerged for conceptualization ever since the seventeenth century. That was the time when political theorists were first "confronted with a hitherto unheard-of process of growing wealth, growing property, growing acquisition" and thus turned "to the phenomenon of a progressing process itself, so that . . . the concept of process became the very key term of the new age as well as the sciences, historical and natural, developed by it" (105).

Labor is the only activity into which we can translate, and which corresponds to, the life process in our bodies that we know through introspection. The discovery of introspection in philosophy coincided with that of process in the natural sciences. And modern philosophies of labor, followed by philosophies of life, have equated productivity with fertility; only, the philosophies of life have lost sight of labor, which is the activity needed to sustain the life process — and labor, having become more effortless than ever before, has indeed become "more similar to the automatically functioning life process" (117).

To labor and to consume are so overwhelmingly the activities in whose terms we conceive of our lives that every other activity, all that is not "making a living", is a "hobby". This dichotomy is anti-

pated in the several types of modern labor theory, all of which contrast labor with play. And leisure, which has increased and spread by the mechanization and "division" of labor, has engendered the crucial problem "how to provide enough opportunity for daily exhaustion to keep the capacity for consumption intact" (131). Still, no matter what forms labor takes on, labor is in the realm of necessity. Its elevation to unprecedented status is the elevation of necessity to such status.

IV

Homo faber, the maker of things and objects — in contrast to the man of action — "is master of himself and his doings" (144). Labor, as compared with making, has no beginning or end, and action inevitably involves others. *Homo faber* is different from both laborer and man of action also in distinguishing between means and ends in respect to his activity, making or fabricating. Both he and the laborer may use either tools or machines. The *problem* of the machine, however, has arisen with the function of the machine in the service of labor, rather than in the service of fabrication. Since everything, natural or man-made, enters the conditions of man's existence, the problem of the machine is not one of "adjustment": man adjusted himself to the machine the moment he designed it (cf. 147). Rather, the problem of the machine arises from the fact that "as long as the work at the machines lasts, the mechanical process has replaced the rhythm of the human body" (147). And if the present stage of machine technology channels "natural forces into the world of the human artifice, future technology may yet consist of channeling the universal forces of the cosmos around us into the nature of the earth" (150). The tools and implements of *homo faber* primarily serve to erect a world of things. The question of machines today is not so much whether we are their masters or their slaves as whether they still serve such a world or whether, "on the contrary, they and the automatic motion of their processes have begun to rule and even destroy" (151) it.

It could be that the dialectic which governed the replacement of *homo faber*, man the maker, by *animal laborans*, man the laborer, was released by the failure of Western man to realize that, "while only fabrication with its instrumentality is capable of building a world, this same world becomes as worthless as the employed material . . . if

the standards which governed its coming into being are permitted to rule it after its establishment" (156). That is, the means-end scheme, which controls the making of the world of things, does not control it once it is made. Instead, it controls the public realm of *homo faber*, namely, the exchange market (cf. 160). The exchange market is the arena of values in the authentic historical and hence the only legitimate sense of this term:

"The much deplored devaluation of all things . . . begins with their transformation into values or commodities, for from this moment they exist only in relation to some other thing which can be acquired in their stead. Universal relativity, that a thing exists only in relation to other things, and loss of intrinsic worth, that nothing any longer possesses an 'objective' value independent of the ever-changing estimations of supply and demand, are inherent in the very concept of value itself" (165-6; cf. 235, n. 74).

The reason that every man-made thing once made (as contrasted with the thing-in-the-making) transcends the means-end scheme is that it "must appear, and nothing can appear without a shape of its own". Consequently, everything in some way transcends "its functional use, and its transcendence, its beauty or ugliness, is identical with appearing publicly and being seen" (173).

V

Man may be human without laboring (but letting others labor) or working (but enjoying the things made by others); but a "life without speech and without action . . . is literally dead to the world; it has ceased to be a human life because it is no longer lived among men" (176). To speak and to act is to insert ourselves into the world, not by necessity, as when we labor, nor by utility, as when we work, but by virtue of that beginning which was our birth; it therefore is like a second birth (cf. 177 and n. 3).

Speech plays a much greater role in action than in any other human activity, where it rather has the function of communication. Indeed it must not be confused with communication, although in modern society, including much of modern social science and philosophy, the two are often, if not characteristically, no longer distinguished — just as labor and work, and behavior and action, have lost all clear differentiations. Unlike communication, which, along with making, is

governed by the means-end scheme, speech and action are not. They disclose the person who engages in them and is willing to risk being disclosed. This, however, he can do only in the public realm: neither the doer of good works nor the criminal can, since their deeds and words must remain hidden. And neither is such revelation possible when people are for or against each other, rather than simply together, for then, speech and action become means to an end, as, for instance, in war

Only a man's biography — the story of which he is the hero — tells us *who* he was or is; hence "we know much better and more intimately who" Socrates was "because we know his story, than we know who Aristotle was, about whose opinions we are so much better informed" (186).

We cannot know "human nature" or who man is because *his* story is "history", or "the storybook of mankind", and mankind is an unidentifiable agent (cf. 194). Even the individual agent cannot know the meaning of his action: it can be known only by the storyteller, who has the advantage of hindsight, as we have just seen. The reason for this is that action is "boundless" and "unpredictable": "it acts into a medium where every reaction becomes a chain reaction and where every process is the cause of new processes" (190). The resulting frailty of human affairs was transcended by the Greek *polis*, which assured "the mortal actor that his passing existence and fleeting greatness . . . [would] never lack the reality that comes from being seen, being heard, and, generally, appearing before an audience of fellow men" (198).

That which keeps the public realm, that is, the space in which acting and speaking men appear, in existence, is *power*. Power passes away when it is not actualized, and it "is actualized only where word and deed have not parted company, . . . where words are not used to veil intentions but to disclose realities, and deeds are not used to violate and destroy but to establish relations and create new realities" (200).

Power, inseparable from action and from politics, is unknown to *homo faber*, who is convinced that man's products may be more than himself, and to the *animal laborans*, who believes that life is the highest of all goods. Power, instead, accrues to the man of action, for whom "the greatest that man can achieve is his own appearance and actualization" (208).

Because action is unpredictable in its outcome, is irreversible (while products can be destroyed), and is anonymous in its authorship,

the attempt to substitute making for action is as old as recorded history. Fundamentally, it is the attempt to have one man, modeled after *homo faber*, make political life. This attempt is at the bottom of all arguments against democracy — that political form which most fully recognizes the human condition of plurality — and thus is “an argument against the essentials of politics” (220) itself.

We have seen that in the modern age the seeming elimination of labor has resulted in work being performed in the mode of labor, and the products of work, that is, objects, being consumed like consumer goods. Similarly, the attempt at eliminating action and replacing it by making has resulted “in channeling the human capacity for action . . . into . . . a [new] attitude toward nature” (231), into which we have begun to act.

Still, the fact that man is both free and not sovereign, that is, that he can act, start something new, and yet cannot control its consequences, does not justify the “existentialist” conclusion that human existence is absurd. For action is not only irreversible, it is also forgivable; and the remedy for its unpredictability is “the faculty to make and keep promises” (237). Forgiveness terminates a chain of action and reaction, where vengeance promotes such a chain; and forgiveness is as revelatory as action itself. The force of mutual promise, or contract, is the power that keeps the public space in which people can act together in existence. What is mastership “in the realm of making and the world of things” is “sovereignty in the realm of action and human affairs”; but while the former “is conceivable only in isolation”, the latter “can only be achieved by the many bound together” (245).

VI

Throughout most of the portions of her book that I have expounded up to now, Arendt has written in an historical vein more implicitly than explicitly. She has told us, for instance, of the (historical) rise of society, and of the (historical) confusion between private and public, property and wealth, behavior and action, labor and work, consumption and use, values and a great many things, communication and speech, power and strength, force, or violence, action and work (and, as we shall see in a moment, truth and truthfulness). In the last part of her book, “The *Vita Activa* and the Modern Age”, she faces, equipped with the frame of reference with which we have

acquainted ourselves, more explicitly the nature of the modern age and its history.

The modern age is characterized by "world alienation", or the loss of a common human world. In respect to social life, the first phase of this age was marked by the cruelty toward ever-increasing numbers of laborers deprived of family and property, that is, of a place in the world; its second stage, by the rise of society which replaced the family as the subject of the new life process. In physics and philosophy, the center of the universe, at one time the earth, later the sun, has disappeared in the Einsteinian "centerless world view" (263). We have transcended the age of "natural science", "which looks upon nature from a universal standpoint and thus acquires complete mastery over her", and have entered that of a "universal science", "which imports cosmic processes into nature even at the obvious risk of destroying her and, with her, man's mastership over her" (268). Our thought is still dominated by the Cartesian doubt and its two nightmares: that reality may be a dream, and that an evil spirit, rather than God, rules the world — a devil who has created "a creature which harbors a notion of truth" but which "will never be able to reach any truth, never be able to be certain of anything" (277). Thus, in religion, Protestantism has resulted in the loss of the *certitudo salutis*; and in regard to cognition, modern man has lost the certitude of truth (and has substituted for it the ideal of truthfulness). Introspection, or "the sheer cognitive concern of consciousness with its own content" (280), has come to dissolve objective reality into subjective mental processes and has transformed common sense into "an inner faculty without any world relationship" (283). Modern science deals exclusively "with a hypothetical nature" (287); scientific and philosophical truth have parted company; and "the philosopher no longer turns from the world of deceptive perishability to another world of eternal truth, but turns away from both and withdraws into himself" (293).

The loss of a common human world was first indicated by the elevation to predominance of making and fabricating. Man who made instruments and experiments no longer asked "what" or "who" but only "how". The objects of his knowledge no longer were things or eternal motions, but processes; no longer nature or the universe, but their history. Ever since Vico, the despair of human reason made itself felt in the conviction that only man-made things were understandable to man.

Yet there was a second shift or reversal: *homo faber* was replaced by *animal laborans*; making by laboring; means-end-product by process; the principle of utility by that of the "greatest happiness of the greatest number", that is, by the principle of life itself. The reason for this second reversal, a reversal within the *vita activa*, is the very medium within which it took place. This medium was the Christian society with its traditional immortality of the individual life, as against the pre-Christian immortality of the body politic. This medium, however, had been secularized, and secularization had deprived man of faith in individual immortality, such as had still been believed in during the Middle Ages. Now, the one thing of potential immortality "was life itself, that is, the possibly everlasting life process of the species mankind" (321). The concept of "life" unifies all processes, from subatomic through human to terrestrial and galactic. And the reason

"... why the behavior of the infinitely small particle is not only similar in pattern to the planetary system as it appears to us but resembles the life and behavior patterns in human society is, of course, that we look and live in this society as though we were as far removed from our own human existence as we are from the infinitely small and the immensely large which, even if they could be perceived by the finest instruments, are too far away from us to be experienced" (323).

Nevertheless, men keep on making things and have not even lost the capacity to act, although this capacity is largely limited to the scientists. The scientists, however, act into nature by releasing processes. Their action, therefore, lacks, "the revelatory character of action as well as the ability to produce stories and become historical" (324).

In line with both the premodern and modern tradition, Arendt has omitted thought from her reconsideration of the *vita activa*. But she concludes her book with a few remarks on it. "As a living experience", she writes,

"... thought has always been assumed, perhaps wrongly, to be known only to the few. It may not be presumptuous to believe that these few have not become fewer in our time. This may be irrelevant, or of restricted relevance, for the future of the world; it is not irrelevant for the future of man. For if no other test but the experience of being active, no other measure but the extent of sheer activity were to be applied to the various activities within the *vita activa*, it might well be

that thinking as such would surpass them all. Whoever has any experience in this matter will know how right Cato was when he said: . . . 'Never is he more active than when he does nothing, never is he less alone than when he is by himself' " (325).

In her end she thus comes back to her beginning: "What I propose", she then wrote,

" . . . is a reconsideration of the human condition from the vantage point of our newest experiences and our most recent fears. This, obviously, is a matter of thought, and thoughtlessness — the needless recklessness or hopeless confusion or complacent repetition of 'truths' which have become trivial and empty — seems to me among the outstanding characteristics of our time. What I propose, therefore, is very simple: it is nothing more than to think what we are doing" (5).

Two : Interpretive

We should miss the nature of Arendt's book, were we not to analyze it both as communication and as thinking. In respect to the former, we would examine two facets: factual statements and theoretical claims.⁴ For purposes of this paper, I disregard Arendt's factual statements, although many of them, especially such as are contained in her extensive comments on Plato, Aristotle, Marx, and other thinkers, warrant sharp scrutiny. Nevertheless, they are of secondary importance in light of the aim of this paper, which is to "bring home" the significance of Arendt's book for contemporary sociology. As I see it, this significance lies, above all, in the exemplification of an attitude toward history and our time. The power of this attitude is related to the viability of the theoretical claims which Arendt advances in its behalf and, to be sure, but more indirectly, to the correctness of the factual statements which, in turn, she makes in support of her theoretical claims. My interpretation, which omits the factual statements, thus is incomplete, but incomplete, I think, in regard to the relatively least important part of Arendt's work.

By "interpreting" this work (to refer to the title of this second part of my paper), I understand commenting (1) on its theoretical claims, which, along with its factual statements, here disregarded, make it up as a piece of communication; and (2) on its attitude, which, I hold and shall try to show, far exceeds the author's factual and theoretical concerns and reveals her to us as the thinker she is. In order to appreciate this second task of interpretation, we should recall, to

paraphrase Arendt (170–172), that thinking is autonomous, self-contained, without end (that is, without purpose, as well as without terminal point), while communication, like cognition, is patterned after the means-end scheme, Thinking, we go on, tries to accept no distinctions man has ever made (including the many Arendt makes); it is a maelstrom which engulfs whatever ordering the world may exhibit; it shows man at his most unconditional, and this means, at his most practical — as open and exposed to the world as he can be, with no holds barred, no outcome certain, even the fact of any outcome at all uncertain. Thus, to think is the extreme meaning of the practical reaction, including that of the sociologist, to Arendt's book.

As she says with Cato, however, to think is to be by oneself, and to speak, and certainly to write, is to add an element of making — an element which increases in this order — to thinking. And when in the following I enumerate some of the propositions I wish to submit for inspection, perhaps even thinking, by others, it is a sure sign that I am submitting cognitions and tentative outcomes, that is, interruptions of thinking yielded to under its onslaught, and made, but only somehow, into entertainable propositions — I cannot do the impossible, submit thinking itself.

The first part of these “halts” in thinking — all of which I consider relevant to the social scientist — concerns (we recall) theoretical questions, including definitional and methodological ones, some of them requiring answers in order to decide the items in the second section, which are practical.

(1) Theoretical

(a) *The Relations Among the Conditions of the Three Activities of the Vita Activa.* — To say that the human condition of *labor* is life itself means that man (though not necessarily every individual man) must labor if he wants to live. To say that the human condition of *work* is worldliness means that man (with the same qualification) must work if he wants a world beyond nature. Yet, of course, he must live in order to be able to work, but must labor in order to live. It thus appears that the two human conditions, life and worldliness, are not coordinate and mutually irreducible but, rather, hierarchical: labor makes life possible, which in turn makes work possible; but while life is possible without work (though not without labor), work is

possible only if there are labor *and* life. Furthermore, labor is biologically necessary, while work is not.

As to "plurality", the predicate "condition" does not mean what it does in the cases of "life" and "worldliness". Man can choose not to live and not to be worldly, but he cannot choose not to be plural: plurality, as the word says, is not an individual trait, and its abolition is the abolition of mankind, which, on the contrary, is left intact by both the individual's death (including suicide and murder) and other-worldliness (in whatever sense of this term). Plurality, in fact, is the condition both of the denial of life through man-made death, whether suicide or murder, and of other-worldliness. Durkheim has shown this in respect to suicide; it is obvious in the case of murder; and other-worldliness, even if in the form of withdrawal from mankind, is a withdrawal, that is, an activity "that goes on directly between men" and was preceded by other such activities.⁵ Plurality, in fact, is the condition without which man cannot survive long enough to reach the maturity needed for choosing either death or other-worldliness; even if there should be authenticated cases of feral children (I am not aware of any), they would not be of human children, who, like many other animals, need adults of their species in order to grow up.

It turns out that all three of the human conditions — life, worldliness, and plurality — corresponding to the three human activities of labor, work, and action, are not coordinate and mutually irreducible. For neither life nor labor is possible without the other; both life and labor are necessary prerequisites of work, but not vice versa; and neither life nor worldliness is possible without plurality, which, therefore, is necessary not only for action, but for labor and work as well.

(b) *Labor and Work*. — Labor is private, necessary, repetitive, and for consumption, while work is public, spontaneous, with a beginning and an end, and destined to last. Yet we have seen that work can be performed in the mode of labor, and the products of work can be consumed like consumer goods. This suggests that the above predicates of labor (private, necessary, etc.) and work (public, spontaneous, etc.) are not intrinsic to these activities but dependent on man's attitudes; that a man is not laboring or working by virtue of what he does but by virtue of what he thinks he is doing. Thus, an assembly-line "laborer" in an automobile factory labors if he expects that the car into the production of which his motions go will be consumed; but he works if he thinks that he contributes to making a use object. And

whether an item is a consumption good or a use object likewise appears to depend on the attitude that those in contact with it have toward it. Thus, if I use my car as a vehicle in the service of my everyday life, including my occupation, it is a use object. — But what can it mean to say that “I consume it”, that it is a “consumer good”, other than that I “feed” it, as it were — not into my stomach but into my system of thrills or prestige? Something like “voraciousness”, whether literal or figurative, thus appears to be the attitude which determines an item’s status as consumer good. The only other criterion that comes to mind for distinguishing between use object and consumer good is even more inarticulate. This is the matter of the time that an item is to last: if brief, it would be a consumer good; if longer, a use object. This time element does enter into Arendt’s distinction — things, for instance, have “durability”. But by this criterion, how long must a car last to transcend its status as a consumer good and become a use object?

The result of our questions seems to be that labor and work are ideal-typical extremes of a continuum which, as we move, in this order, from one to the other, decreases in privacy, necessity, repetitiveness, consumptive character, perishability, as well as in the intent for the product not to have these characteristics — or to put it positively, which increases in public nature, spontaneity, beginning-and-end character, durability, as well as in the intent for the product to have *these* characteristics. In other words, labor and work are, on this view, not different in kind but different in the degree to which they possess, not one, but a number of characteristics.⁶

But surely, this is not what Arendt means. A reversal such as that from *homo faber* to *animal laborans* is not a quantitative affair, or at least what Arendt wants to convey is the change in *kind* from one to the other, never mind what difficulties of formulation and demonstration a more careful conceptual analysis might uncover. Hers is a practical, a “rhetorical” concern; and indeed, “kind” or “quality” is, above all, an inevitable category of practice. At this point (and we shall see others), Arendt’s theory appears in the service of practice but much in need of clarification; she wants “to think what we are doing” — the thinking and cognizing are for the sake of the doing.

(c) *Action*. — Like work, action is public and spontaneous; like work, it has a beginning, but it has an end in a sense other than in which work has an end; and, again like work, it produces lasting things, its “stories”, which correspond to work’s use objects. While

work is ended when its purpose is achieved, Arendt would probably admit that an action, too, has a terminal point, which is also a feature of the empirical (though not the analytical) referent of Parsons' "unit act".⁷ But this is quite irrelevant compared with the fact that action is "boundless" and "unpredictable". It is difficult to understand the difference between these two attributes unless the first means "infinitely interpretable". This reading is suggested as the result of juxtaposing the "boundlessness" of action with the idea that action, by virtue of being free and in this sense an absolute beginning (cf. 177 and n. 3), is part of a unique person who, being unique, cannot be exhaustively or definitively interpreted — cannot even tentatively be interpreted before his death, that is, before he ceases to act, before we have all his actions whence to make our stories. This reading, however, would make the story, or historiography, of any people not yet extinct impossible — not only that of mankind, as Arendt says. And the death of a people, say the Roman people, would, in turn, be a matter of interpretation: we might have no historiographies at all.

The meaning of "boundlessness" as infinite interpretability thus must not rest on the impossibility of historiography — after all, there *is* historiography — but on the initiatory character of action. I suggest that this character refers to the uniqueness of action, its sheer "thereness", to the total absence of the fabricating, making, working, purposive element. To put it differently, action, like thinking, is as pure a state of being or activity or human elementarity as we know of, and for this reason we can "make" anything out of it, not just one thing — exactly as in the case of thinking. The only difference between action and thought, in fact, is what might be called externalization or objectification, which is present in the former but not in the latter.

Arendt also calls action "unpredictable". That is, it is unpredictable in both its occurrence and its consequences. Let us take these two subjects of unpredictability in this order. Does the first mean that when a person does something which I predict, he is not acting but, say, behaving? And does it mean that if he does something other than I predict he is acting? I predict that my friend will show up at noon. He does, hence, he does not act; he does not show up, hence he acts. But this, of course, cannot be Arendt's meaning. Might it be that we have action when something occurs other than any of the predictable alternatives that we can entertain on the basis of our best knowledge? I can predict a large number of things as alternatives to my friend's showing up; but he will have acted only

if instead of keeping his appointment with me he does something I could not have predicted. But this is tautologous; it merely says that the unpredictability of action means the unpredictability of action; and this again cannot be what Arendt means. Perhaps "unpredictable" does not mean "not predictable" at all but rather "that in respect to which the question of predictability or unpredictability is irrelevant". That is, in action, the dimension of predictability-unpredictability is overshadowed by another dimension: once more, by its uniqueness, "thereness", novelty, initiatory character. The nature and location of the criterion of this characteristic remain to be worked out; but we shall see — on the basis of the fact that action presupposes plurality in a sense in which labor and work do not — that this criterion is located neither in the agent nor in the witness, alone.

What about the unpredictability, now not of the occurrence, but of the consequences of action? Since we do not know all causal laws, we cannot predict these consequences — but for the same reason, we cannot predict the consequences of anything, including labor and work. Does Arendt mean that the regularity of labor and work ease their predictability and that the irregularity (if this term be permitted) of action makes its predictability more difficult? Probably not, since this, again, would be a quantitative matter, to be discarded for the same reasons that we thought we had to discard it in reference to the distinction between labor and work. Here, too, we may surmise, the unpredictability of the consequences of action derives from its novelty: action brings something new into the world, and for this reason it is intrinsically unpredictable — not only difficult to predict. If "predictable" and "unpredictable" be understood as causal concepts, Arendt would probably mean, not that action is not part of any causal matrix, but that because of its novelty we cannot anticipate the emergence of the cause "action", nor its effect. In this sense, it is a "miracle", as man as man of action himself is.⁸

The counteragent of the boundlessness of action is that it can be forgiven; and of its "unpredictability", that it can be hedged in by promise. But the forgivableness of action raises problems about our interpretation of its boundlessness and unpredictability. For if these are interpreted as, respectively, infinity of interpretability and novelty, what then is to be forgiven about action? The unprecedented, unique, unforeseeable lies outside the realm of forgiving or not forgiving, which is the realm of ethics. It is at this point that we must remember plurality

as the condition of action, and we realize that there *are* limits — precisely those set by plurality — to the uniqueness, sheer “thereness” of action. Only when action exceeds these limits imposed by plurality — and recall, “plurality” is not “society” — does forgivability become a criterion applicable to it. We thus realize that action is, as it were, an experiment with the ethical character of plurality such that, for instance, the prophet’s action needs no forgiveness, although he transcends the ethics of plurality as grasped prior to his emergence, whereas a Raskolnikov does. It is clear that Arendt’s conceptions of ethics, plurality, and action, and their interrelations, call for much more explicit treatment than she has given them.

In regard to “the faculty to make and keep promises” as the remedy for the unpredictability of action, there is a related difficulty if we would preserve action’s uniqueness. For, contract, or the force of mutual promise, surely follows the means-end scheme, and thus is more a case of making than of acting. Here again, the way out of the apparent contradiction must lie in the fact that only *that* promised doing and not-doing is action which is limited, not by a purpose, by something to be achieved or avoided, but exclusively by the ethics of plurality as such.

While this, too, obviously needs explication, it will be clear that the criterion of the uniqueness or novelty of action, that is, the question when a given doing is or is not action, lies *both* in the agent and his witness: this criterion itself is developed and applied by the plurality such as it exists at the moment when this plurality confronts the doing. It thus is a practical, and both a collective and a historical, criterion.

(d) *Public and Private*. — Action, we have heard, is public, whereas labor is private. Here, too, we must argue that these might appear to be matters both of degree and of attitude. First, how public is “public”, and how private is “private”? We do know from experience and observation of others in our society and in other societies that here, again, there is a continuum which ranges between the poles of a completely public and a completely private character. This is true in either of the two senses of “public” which Arendt distinguishes — the adjective (“public” is what “can be seen and heard by everybody . . . by others as well as by ourselves”, where the quantitative element is implied in the question of *which* others and *which* ourselves) and the noun (the man-made world common to men, where the quantitative element is implied in the question of *which* men, which how-brought-up men). And the answer to the quantitative question is the attitude or

intent of the doer, who wants his doing to be public or private within the limits that he intends to draw.

But we can only repeat what we said after we brought out the quantitative and attitudinal character of work and labor. We did not think then that Arendt was engaged in an ideal-typical construction of a continuum but in persuading us of the reversal from work to labor, from *homo faber* to *animal laborans*. Similarly, now we think that she is concerned with driving home the emergence of the household as the organizing principle of society, the rise of the private to public recognition. And again, the theory in the service of her practical concern needs clarification.

And what about this identification of privacy with the household and with necessity? This may be an apt description of the *polis* but surely does not cover all meanings of privacy, notably the privacy of feeling (rather than only of feelings which issue into bodily activities, to which Arendt alludes [73]) and of thinking, which she mentions (325). The "necessity" of labor as a condition of life (as well as vice versa) is one thing; the "necessity" of a feeling which one is overcome by is quite another; the "necessity" of the very thinking that is begun in freedom is different yet. Nevertheless, all three are private: my having to make a living is private, my love and hatred are private, my thinking is private. What makes us call these three different things, all of which are "necessary" in three very different senses of the term, by one name, "private"? Surely, only the first has anything to do with the household, and "necessity" has such different meanings for each of them that to take it as their common characteristic strikes one as playing with words. "Private" itself means something different in the three cases. In the first, it means, precisely, "of no public concern", of no relevance to action or to history — a technical condition without which man can neither work nor act. In the second case, in regard to feeling, it means, not that man cannot work or act unless his bodily needs are met, but that he cannot work or act unless he feels; that his feelings become of public concern only in so far as they become transformed into publicly inspectable works or deeds. "Private" in the first case refers to a necessary condition of work or deed, but in the second case only to a possible source. In the third, finally, in regard to thinking, "private" refers to that solitude in which man is less alone than when he is with anybody, for then, being most human, he is most part of mankind. The fact, thus, that we do apply the term "private" to all three — making a living,

feeling, and thinking — suggests that we see man at once as an organism, as a potential maker and doer, and as a member of mankind.

(e) *Politics and Power*. — To be able to act demonstrates man's freedom. In our discussion of action, we found it necessary to limit this freedom by the ethics of plurality; but a further limitation is imposed by Arendt's locating the realm of freedom in politics, that is, by her tendency to equate action with political action (e.g., 220).⁹ But freedom in the sense of starting something new by virtue of one's birth (177) and in the political sense of starting something new (by virtue of one's birth) in behalf of one's society surely is not the same thing; nor is it clear why man can act (the prerequisite for disclosing himself) only if he is with, rather than for or against, other men (180). An unanalyzed infatuation with the *polis* or with an image of the *polis* may be at work here; but perhaps there is something of more than biographical interest, namely, the fact that, *polis* or no, man's freedom can be realized only in common with other men. This means that what we have vaguely called the ethics of plurality becomes somewhat more concrete; it comes to mean that truly to act in freedom is to act for mankind's good. This, however, is difficult to reconcile with the discussion of the irreversibility of action (220), its inevitable incurrence of guilt (223–35), and the attendant distinction between man's freedom and his non-sovereignty (234–35). In Arendt's conception of political action, we have the desire for the demonstration of the continuity of mankind, rather than the demonstration itself, as we do have it in the remembering of etymology,¹⁰ that is, of our speech and its history.

And what is to be understood by "political" itself is not clear either. "The Latin usage of the word *societas*", Arendt writes (23), "also originally had a clear though limited political meaning; it indicated an alliance between people for a specific purpose". And she gives examples — *societas regni*, *societas sceleris*, etc. Is it the character of what in contemporary sociology is often called an interest group that makes a group political? Or is it this character that makes it political, rather, in a limited sense? Surely, an interest group has a purpose and, to achieve it, uses certain means — all of this has the earmarks of making, not of action. Perhaps the solution of the puzzle is that to the extent that the effect of the group's activities — to rule, to commit a crime, to do business — enters the surrounding plurality, the group is political, even if its mode of procedure is work rather than action. If so, it would have clarified matters if Arendt had made

this argument explicit, although explication might not have validated it. At any rate, it would parallel our understanding of promise as action, despite its contractual, that is, means-end, character (at the end of *c* above).

That which keeps the public realm in existence, we are told, is power, the attribute of the man of action, but not of the maker, for whom his products, nor of the laborer, for whom life, is the highest good. Power, apparently, is engendered only by public action, by action publicly perceived, taken up, carried on, and transformed. Power is the power of freedom, of beginning, it would appear, and it is, as we saw, "to an astonishing degree independent of material factors, either of numbers or means"; it is a potentiality which can be actualized but not fully materialized (200), and unlike force, Arendt says elsewhere,¹¹ it grows, rather than decreases, if divided. Power, apparently, is the contagious humanness which action discloses; there is nothing that man cannot do if the spirit moves him. Unfortunately, here again, the practical-rhetorical nature of Arendt's enterprise is indicated, among other things, by the imprecision of the concept. Applying it, as she does elsewhere, to the power of the federal government of the United States which, she claims, is increased, rather than diminished, by the power of the states, she finds herself recommending that the matter of school segregation or desegregation be left in the private sphere which, apparently, is meant to be championed by the states rather than the federal government.¹² Such practice suffering from unclarified theory complements the withdrawal from practice that appears characteristic of much contemporary social science. If the latter evinces what Arendt calls world alienation, perhaps the former betrays the desire for a world *à tout prix* — in her terms, it evinces not so much power as violence.

(f) "*Society*" and "*Behavior*". — What are the social scientists' "societies" that existed prior to the rise of society, and when did society arise? The two questions are interrelated. That is, the fact that Arendt does not propose a term to designate pre-society "societies" is connected with her failure to be clear on the time period in which society itself emerged. We have heard that this happened in the modern age, with "the rise of the 'household' . . . or of economic activities to the public realm" (33), when it, society, replaced the family as the subject of the new life process (256). Although society is a modern affair, and the modern age began in the seventeenth century (6), still Arendt speaks of the medieval "society of the faithful"

(31). Perhaps this is no contradiction but an easily corrected failure to explicate the beginnings and the history of society. But if so, this failure, in turn, may have something to do with the importance Arendt gives to etymology. Is the historical and sociological significance of Livy's *societas regni*, of Cornelius Nepos' *societas sceleris* of Aquinas' *societas* of risk-sharing investors, of the medieval society of the faithful, of Locke's society of property owners, of Hobbes' acquisitive society, of Marx's society of producers, of our society of job-holders, of the totalitarian laboring society (23, n. 3; 31) even comparable except in a distorting and misleading sense? Theoretically, it is clear that a much more careful tracing of continuity and change is needed, with a terminology accurately designating the results of such analysis; and perhaps even prior to this, one would have to decide on what to call all groupings that do not figure in this enterprise. Hence the interrelation between the vagueness concerning the rise of society and the failure to give pre-society "societies" a name or names. But it would also appear that Arendt has here succumbed to one of the dangers of etymology, namely, the tendency to assume that the same word means, if not the same thing, at least something related. The question of a defensible attitude toward etymology will come up in our consideration, below, of what she has to say about "values".

Similar observations must be made in respect to "behavior", which, in society, as we have seen, replaces action. The questions that arise are: (1) What other than labor, work, act, think, and contemplate does or can man do, and what are the relations between his activities in the *vita activa* to whatever these other things may be? And (2) what were people engaged in (except in laboring, working, acting, thinking, and contemplating) before, with the rise of society, they started to behave? Both questions parallel one of those we raised about society itself, namely, the question concerning the lack of nomenclature for pre-society "societies", which corresponds to the lack of terms for both pre-behavior "behavior" and human doings other than those discussed. It is tedious, by now, once more to call attention to the practical nature of Arendt's undertaking — here, to her desire to "carry home" the nature of our society, the nature of our behavior — as well as to her theoretical inadequacies.

Intrinsic to Arendt's observations on society, of course, are her observations on labor. Labor, we are told, is the only activity into which we can translate, and which corresponds to, the life process in

our bodies that we know through introspection — hence (“hence”, it would seem, both logically or psychologically, and historically) the modern labor philosophies and their succession by the various philosophies of life (117). But neither her argument, nor experience, shows that labor, rather than thinking (325), is the only activity that corresponds to the introspectively perceived life process, and is the only activity into which we can translate this process. Is she not missing something to clinch her argument — a sociological, rather than a psychological feature? Might this feature not be the distrust of thinking, rather than the objective impossibility of discovering thinking through introspection (recall Descartes, otherwise so much and importantly drawn on by Arendt), which is relevant if we would account for the availability of life, and the fascination by it, in the various philosophies of life? And in respect to labor philosophy, might a quite different feature not be less intellectual than social and political, lying in the area, to use a Marxian term, of class struggle, of shifts in the relative claims and powers of “labor” and “capital”?

(g) “*Nature*”. — Arendt distinguished between “nature” and the “cosmos” or “universe” (150, 268). Nature is not only that which is not man-made, since this applies also to the universe; but it is, most explicitly suggested by the equation of “terrestrial and ‘natural’ laws” (268), the earth. Hence Arendt’s distinction between “natural” and “universal” science, the former looking at nature — read “the earth” or “man’s traditional habitat” — from a universal standpoint, the latter importing cosmic processes into nature itself (*ibid.*). We have seen the considerations into which this distinction enters (VI, above) and thought it appropriate to record it explicitly, rather than having to glean it from widely scattered passages in Arendt’s book.

(2) Practical

The matters discussed in the section just concluded were theoretical, that is, largely, questions of conceptual and, by implication, factual clarification. In our efforts to answer these questions, we found ourselves led back, again and again, to Arendt’s attitude and concern, which we have called predominantly practical and rhetorical. We have now come to a point in our preoccupation with her book at which we ourselves take a similar, practical attitude, which focuses on what we must do — what we, as social scientists, must do.

Our focal points will be (a) behavior and action, (b) value, (c) the means-end scheme, and (d) man's historicity and dualism.

(a) *Behavior and Action*. — We have commented on some of the difficulties inherent in Arendt's use of these terms. Now we disregard these theoretical matters and, instead, consider the meanings of "behavior" and "action" as used by contemporary social scientists, on the one hand, and by Arendt, on the other. We have seen that Arendt holds "behavior" to be a historically specific term, which it is not in contemporary, nor for that matter in past, social science. There are other differences, however, between the two usages of both terms, "behavior" and "action". A number of adjectives may serve to suggest them.

(1) Noncommittal-committal. Social scientists do not commit themselves to pass judgment on the merits or demerits of man's activities which they call behavior and action, whereas, we have seen, Arendt does.

(2) Operational-substantive. For them, the terms are what their operational definitions stipulate; Arendt means to get at their nature.

(3) Observational-participatory. The social scientists set up their definitions and then observe the empirical referents of these definitions; Arendt's relation is more to behaving and acting individuals, with whom she is participating, than with the behavior and action abstracted from them.

(4) Theoretical-practical. For social science, the terms are concepts in theory; for Arendt, they refer to human fates.

(5) Nonhuman-human. For social science, "behavior" and "action" can be predicated of any item in the universe, even of concepts (not only of animals, but of prices, rates, telephones); the terms do not refer to exclusively human activities; hence the question concerning their place within a conception of man does not come up, whereas it is central, no matter how implicit, for Arendt.

(6) "Out-group—in-group". Social scientists are not members of the group of people or things that behave or act; Arendt is.

(7) Extrasystemic-intrasystemic. Social scientists do not intend to conceptualize themselves and have the concepts thus obtained enter the system in which the concepts "behavior" and "action" are located; Arendt does.

(8) External-existential. The terms refer to matters external to the social scientists; for Arendt, to characteristics of human existence, including her own.

(9) Neutral-affective. They are affectively neutral both intrinsically and in respect to the social scientists' relation to them; the opposite is true, in both respects, of Arendt's use.

(10) Scientific-humanistic. For social scientists, "behavior" and "action" are abstracted from their "apperception mass"; for Arendt, they are real and of real concern.

(11) Non-normative—normative. In social science, the referents of the terms lack any character of requiredness or obligatoriness; for Arendt, they have such a character.

(12) Systematic-historical. In social science, the terms function, not only in theories, but in systematic theories (of man's social and psychological life); in Arendt, the terms function in a conception, philosophy, or theory of history. They may also be called, respectively, nomothetic and idiographic.

There is also, however, a difference between the distinctions that social science and Arendt make *between* "behavior" and "action". If social scientists make one at all, it is in the form Max Weber gave it, for whom "action" was

"... human behavior (no matter whether external or internal doing, failing to do, or suffering) if and in so far as the acting person or persons connect a subjective meaning with it"¹³

— which includes all but reflexive and other unconscious behavior under the category of action, and within this category makes no contentual distinctions. What the two terms mean for Arendt, and consequently how she distinguishes between them, we saw in the preceding part of this section and in the last paragraph.

It is clear that as sociologists, we have to make a decision in respect to these two competing conceptions — reject or accept them both, or accept the current or Arendt's usage. I have gone to such length in assembling adjectives intended to show the differences between the two (many of these adjectives are synonyms or severally imply each other logically) in order to make the need for the decision more plausible. It is clear that most of the adjectives used to characterize the first of the two alternative conceptions of "behavior" and "action", mark it as the scientific one. To recognize this allows us to formulate the decision we have to make as that between being scientific and theoretical, and not being scientific but, say, practical; although the four alternatives (accepting both, rejecting both, accepting the first, accepting the second) remain open. If we view, as I do,¹⁴ social

science itself to be both theoretical and practical, we must make our decision accordingly: we must choose the first alternative, that is, accept both conceptions.

This decision itself, however, changes them, namely, from a competitive to a cooperative status. The question now before us is: for what purpose, in what situation, must we use one or the other? The most immediate answer is that we use the theoretical conception for theoretical, the practical for practical, purposes. But this answer needs a qualification that is entailed by our analysis of Arendt, whom we found, in her more practical orientation, to fall short on theoretical clarity, and hence on scientific soundness. To reformulate our criticisms, even if in oversimplified fashion: Arendt's practical results would, perhaps not have changed, but been more persuasive or powerful if she had asked how far and in what ways theoretical weakness has influenced them. This observation illustrates the cooperative status of the components. As to the difference between "behavior" and "action", which in the social-scientific usage is negligible, we have been struck by the power of Arendt's propositions, despite their theoretical inadequacies. We must try to make up for them, although here, of course, we can only attempt to suggest the direction which improvement might take.

I propose to preserve the customary social-scientific use of "behavior" — which answers a number of questions we asked about Arendt's use of the term. And what Arendt calls "behavior", then, might be qualified by an appropriate adjective, such as, perhaps, "administered" — but before we can find a more accurate term, an inspection of relevant literature about the difference between the behavior in "pre-society societies" and contemporary society and, depending on the outcome of such inspection, perhaps actual research would seem indicated. In order to read the literature intelligently and engage in research, however, we must also be clear on her use of "society", which is the locus of her "behavior". Here I propose something similar to the proposal concerning "behavior" itself: to preserve "society" in its social-scientific usage, as the most general term to designate "global" human wholes, and to refer to Arendt's society", once more, by a restrictive adjective, possibly the same as the one suggested for "behavior", namely "administered" — but as in that earlier case, corresponding preparatory inquiries are called for.

"Action", because it has a hardly specific definitional status in social science (despite, of course, the anti-behaviorist "action" theo-

rists, notably Parsons), needs more conceptual work to become theoretically tenable. First, it must be distinguished, precisely, from behavior; then, when properly defined, Arendt's "action" must be further distinguished from it — whereas in the cases of "behavior" (as well as of "society"), the first of these two steps had already been taken in social science itself.

Here it helps to recall Weber's typology of action ("traditional", "affective", "purpose-rational", and "value-rational"). If we allow subjective meaning to be one of the requirements with which to distinguish action from behavior, then, strictly speaking, both traditional and affective action are not action but, precisely, behavior — as Weber himself admitted without, however, eliminating them from his typology.¹⁵ We propose to speak, instead (conceptually or idealtypically), of traditional and affective *behavior*. What is left of Weber's offerings are "purpose-rational" and "value-rational" actions. Let us ask ourselves — let everyone ask this of himself — when it is that we think we have acted. We shall probably find that we think we did, either when we did something which at the time or in recall had more than customary meaning, or when we did something that had what we considered, or have come to consider, important consequences — however we may define either "meaning" or "consequences".

If this is correct, we have two quite different criteria of "action": heightened meaning, and important consequences. On the first criterion, for instance, I act when I think (in Arendt's sense of "thinking"), when I write a poem, perform a piece of music — we immediately have several, easily augmented kinds of action: thinking, artistic action, performing action; always provided, of course, that the first criterion is met. The feeling of particular meaningfulness may often be accompanied by a state of heightened consciousness or self-consciousness; or the assignment of meaning may be accompanied by the recall of such consciousness. By the first criterion, decisions, too, that the agent considers especially meaningful are actions. On the second criterion, we have, perhaps more often than not, actions only in retrospective definition; and probably more often than not, we have action chains or processes, rather than single actions. Suppose a sociologist asks himself how he came to be one, rather than something else which he once entertained becoming: chances are that he will recall a number of things he did, rather than any one — that "one thing led to another" — and that in his mind, all of these together make up a sequence the component elements of which he may call

actions, although not one of them may have been particularly meaningful, or a particularly meaningful decision (if he recalls any decisions at all).

Let the most generic term for behavior that is action by the first criterion, meaningfulness, be "significant action"; we have, almost at random, already listed several types of it (thinking, artistic action, performing action). Let the most generic term for behavior that is action by the second criterion, important consequences, be "consequential action".

"Meaningfulness", clearly, is a subjective criterion. But again we can follow Weber's procedure¹⁶ by introducing the distinction between analyzing *particular* cases that, if distinguished by heightened meaning felt or recalled about them, are actions, and cases *typically* not so distinguished, which we therefore list under "behavior". Thus, the fact that yesterday Smith's piano practice was especially meaningful to him does not make American piano practicing an action since, in reference to the United States (during a certain period), to practice the piano is for certain people (types of people) traditional, and hence falls under the category of behavior. Similarly for consequential action. Aside from the fact that Harry's getting married was particularly meaningful for him, it is likely to have had considerable consequences for him, his bride, and perhaps others. But again this does not make "marrying in the United States" an action; in reference to the United States, it is, once more, behavior.

We should note that, unlike meaningfulness, for whose presence or absence we only have the agent's word (although we can, within limits, check on his statement), "important consequence" is a criterion that can also be applied by the observer — most rigorously if it is understood in the causal sense of the term.

In short, whether or not a piece of behavior is an action depends (a) on its intrinsic criteria, meaningfulness and consequences as perceived by the agent, and (b) on the social unit or period in reference to which the analyst considers it.¹⁷

What about Arendt's "action" in the light of these suggestions? The element of special meaningfulness is contained in her associating action with freedom and disclosure. And if we reconsider the "unpredictability" of her "action", we discover yet another dimension of this term (which caused us so much difficulty) — namely, that "action" is unpredictable in the sense that it may have "important consequences". In our terms, Arendt's "action" would appear to be such

by virtue of its intrinsic properties — “uniqueness”, “thereness”, “initiatory character”, “novelty”, etc. — just as our “significant action” is such by virtue of the intrinsic property of particular meaningfulness (rightly or wrongly located, as we said, by the agent himself).

But when it comes to predicating this intrinsic property, there is an important difference between Arendt’s “action” and our “significant action”. The procedure in our case is scientific or theoretical (in the sense of finding out what is the case), whereas for Arendt it is practical (in the sense of wanting to know what we must do in consequence of what we have found out). But we must add that she has not told us the full story of what she found out that makes her undertake to persuade us that it is the intrinsic properties she discusses which transform behavior into action. Here we are led back to her theoretical insufficiency.

Within our framework, we shall have to drop Arendt’s properties of action and instead define, as the theoretical parallel to her “action”, that type which is characterized either by a maximum degree of meaningfulness or by maximum consequences (or both). The colloquial term “historical act” connotes both of these characteristics.¹⁸

“Maximum” is up to the interpreter to argue. Theoretically, the scientist finds out and reports what others have considered to be maximally meaningful and maximally consequential actions; practically, he arrives at his own commitment concerning them. In most instances, most people, including scientists, cannot collect the scientific knowledge that would enable them to use it as basis for commitment (they lack time, interest, energy, competence, etc.); instead, they use secondhand knowledge (obtained, for instance, from parents, teachers, books).

(b) *Values*. — The history of the term “value”, and its occurrence in various contexts remains to be written. As far as I know, the term originally meant “exchange value”, which thus is redundant for “value” short and simple. But the referents of the word have multiplied far beyond this clearly demarcated usage — conviction, norm, principle, standard, criterion, preference, rationalization, goal, aim, end, purpose, taste, things important, not indifferent, desired, desirable are surely not all of them. Such expressions as “use values”, “intrinsic values”, “ultimate values” are self-contradictory: if my relation to an item is that of use, it is not that of exchange; something intrinsically “valuable” is not valuable relative to something else; and if something ultimate can be bartered for another ultimate, neither of them is.

To designate the many different things, not all of which have been mentioned, by one term, "value", suggests that they have something in common, precisely the element of exchangeability. Unawareness of this may account for the difficulty encountered by those more self-conscious users of the term who try to define it.¹⁹ And such broad, as it were relievingly broad, definitions as "any object of any interest"²⁰ or "a conception . . . of the desirable"²¹ homogenize, not only all matters toward which one is not indifferent or which are desirable to one, but also all ways in which something may be of interest or desirable. Finally, there is the ubiquitous use of the term and the difficulty, especially, it seems, for academic people, of getting along without it. All of this — the multiplicity and heterogeneity of referents, the vacuousness of definitions, and the compulsive character of the use — betrays the exchangeability element, the means element, in all things to which the term refers, whether we are aware of it or not. "Values" are difficult to distinguish from "valuables".

This means-character accounts for what ever since Weber social scientists have come to call the "irrationality" of values, particularly when understood as ends. On this view, a value or end has no "intrinsic worth" that can in any sense be rationally assessed, and all science can do, as Weber insisted, is to make clear to a person:

"If you take such and such a stand, then, according to scientific experience, you have to use such and such a *means* in order to carry out your conviction practically."²²

In short: if you want X, you must pay Y for it — but X is only one of the many values available in the market, and I can tell you the price of each of them, but not which of them you ought to buy or sell, which is altogether your private business. The "stand" the person asking the scientist takes is private; only the area of means, the commodities or exchange market, is public: it is a conversation between two brokers (a species of *homo faber*), the less experienced one asking, the more experienced one answering.

Max Weber was so experienced that he was tempted to consider a variety of "progress", namely, technical progress, altogether outside the market, that is, to deny "technical progress" status as a "value" and elevate it to that of "fact".²³ Thus, he held that if the historian of art means by progress in art the technical improvement of the materials the painter uses — canvas, pigments, brushes — he does not cease to be a scientist; neither does he when he designates as

progress the advent of perspectival drawing. The reason in the first case is that improved canvas, pigments, brushes make painting more efficient; they are better means to its end. In the case of perspectival composition, the end in reference to which it constitutes progress is complexity of vision: to see perspectively adds a way of seeing to the previously available two-dimensional way.

But here Weber, again, is talking as *homo faber*, for whom the end of technology, that is, efficiency, is beyond analysis, and Weber admittedly could not argue with another type of man for whom efficiency is not the "end" — say, a contemplative man or a man of action or a poet. And Weber did recommend that the term "progress", even if it means "technical progress", be dropped from the vocabulary of the scientist. But he recommended it for the wrong reason, namely, in view of the great difficulty of being quite unambiguous about its purely technical meaning. The legitimate reason is that even this meaning does not transcend *homo faber* and hence the irrationality of his ends, including that of technical progress.

Our analysis of the term "value" suggests that what Weber in effect did was to warn social scientists not to be duped by the market. He allowed them to speculate there, of course, but only after hours, privately. In his own terms, they must guard against values that parade as facts such as "progress" (other than technical), "trends", and "adaptation".²⁴ They should not use these terms at all; but if there is any excuse for the use of "progress", it is in its technical sense, because there is universal agreement on *its* aim, efficiency. Thus Weber verged on getting into the market, after all, both by applying an irrelevant criterion — universal agreement — to establish factual status and by declaring efficiency intrinsic to things.²⁵

The fact that social scientists should in recent years have come to go all out for the "study of values" is quite in line with our analysis: they study what people in the market, as it were, what different kinds of people in different situations, are willing to pay for, and how much they are willing to pay to get something else — "value systems" or "value patterns" are fashioned after economic or financial inventories and marketing habits.²⁶ The sociologist, like the market analyst, keeps himself entirely out of his study; he investigates other people's "values", not his own. The reason is that the student's own "values" are scientifically irrelevant, since they *are* private and irrational. Still, while the scientist does not intend to be part of the system he is studying but to remain external to it, purely theoretical and scien-

tific, we have tried to show that, in fact, he, along with the subjects he studies, is practically involved in a much larger system than he envisages, namely, the time in history where he, along with them, is located — the time that has given his activities meanings that he is not aware of and does not control.

What must we do? The first thing that we must do, I should think, is to eliminate the term “value” from our vocabulary, except for its original sense of “exchange value”. Secondly, every time we are tempted to use the term, we must ask ourselves what we mean — and then say it. What we mean might be one or more of the things mentioned previously — convictions, norms, principles, and so on. Third — and in comparison with this, the two steps listed before amount to no more than an imposed diet — we must change our conception of the world in which we live and practice our profession, and recognize it as a common human world of which we are part; that is, we must recognize, acknowledge, and act on our nature, and hence our scientific enterprise, which is ineluctably both theoretical *and* practical.

We now come to raise the question about a “defensible attitude” toward “etymology” that we anticipated. For in respect to the preceding discussion, the objection may be lodged that it is beside the point, that in its contemporary usage, “value” does, “of course”, not mean “exchange value”, and that everybody knows and understands this. The situation is the reverse of an aspect of that in which we analyzed “behavior” and “action”. There, among other things, we found that in one sense of their meaning, both terms referred to the same phenomenon — what on Arendt’s view is, simply, behavior. To call attention to this makes a plausible case for terminological revision — in this case, simplification. Now, on the contrary, we find one term, “value”, to stand for many different things, and the case for advocating terminological revision, in this case differentiation, would appear to be equally plausible. But the objection anticipated is not based so much on theoretical as on practical argument. It urges — to continue its argument — that “value” is a term which has come to stay in our vocabulary, and that no *theoretical* argument is able to dislodge it; and that our concern is unwarranted, anyway, because we have many other instances of words that have changed their meanings, occasionally even into their very opposites.

The question about a tenable attitude toward etymology that is raised by this argument is the question of what attitude we should take toward linguistic change. The answer would seem to be that in

cases where we know of change and have an arguable interpretation of it, we should act on this knowledge and interpretation. First of all, we should call attention to the change and its nature, as we see it; and secondly, if, as in the case of "values", the change reflects a trend we wish to control and alter — rather than being victimized by — we should proceed to do so. The minimum step in this procedure is to bring our own terminology in order and hence to say more nearly what we mean, instead of what we do not know who means.

(c) *The Means-End Scheme.* — It is fair, if somewhat oversimplified, to say that the users of the means-end scheme in recent sociology — chiefly Max Weber, and developing him, Talcott Parsons²⁷ — do not focus on the question of its area of proper application and, particularly, non-application and non-applicability. Rather, it is a model to which to relate whatever action may be examined. Ends (other than as means to further ends) cannot be analyzed with its help; they fall outside its range of competence and, in this sense, are not subject to rational analysis. It should be noted that whatever modification of the scheme has been introduced by structural-functional analysis, notably in Parsons' own later writings, is no more than that. For the structural-functional analysis of ends changes them into means — means of maintaining, disturbing, integrating, etc., the system in which they are located. And as regards the "end" of the system itself — typically it is its survival — it is an end cognate to the laborer-consumer's view of the world (or nonworld), as we shall presently see.

What those fascinated by the means-end scheme tend to forget is that, to repeat a quotation from Arendt,

"... while only fabrication with its instrumentality is capable of building a world, this same world becomes as worthless as the employed material, a mere means for further ends, *if the standards which governed its coming into being are permitted to rule it after its establishment*" (156, italics added).

Or, as she also puts it, utility is identified with meaningfulness; that is, meaningfulness is lost (cf. 158). *Homo faber*

"... will judge every thing as though it belonged to the class ... of use objects, so that, to follow Plato's own example, the wind will no longer be understood in its own right as a natural force but will be considered exclusively in accordance with human needs for warmth or refreshment — which, of course, means that the wind as something objectively given has been eliminated from human experience" (158).

It is clear that in the means-end scheme, the "wind" figures only instrumentally or functionally. To this extent, the scheme is authored by *homo faber*.

But what happens as we move historically from *homo faber* to *animal laborans* is that to the former's lost understanding of meaning is added the latter's lost understanding even of instrumentality. And, further,

"... just as the implements and tools *homo faber* uses to erect the world become for the *animal laborans* the world itself, thus the meaningfulness of this world, which actually is beyond the reach of *homo faber*, becomes for him the paradoxical 'end in itself' " (155).

This "end in itself" is the "survival" of the structural-functional system. In the society of *animal laborans*, man labors in order to consume — irrespective of how relatively little, and ever less, he may have to labor. For him, there is, strictly speaking, no longer any world, whether of durable things or of speech and stories; both work and action have disappeared, and necessity, the earmark of both labor and its counterpart, consumption, reigns more absolutely than it has ever before in history (cf. 126–35, esp. 133–35).

Let us continue from here. We have spoken of "necessity" once before (at the end of *Id*) — of its different referents when the term is used in connection with labor, with feelings, and with thinking. The meaning of instrumentality, of the means-end scheme, may receive new light once we realize that in relation to it, necessity is located in the connection between means and end, this connection being causal, and a causal connection being a necessary connection. As the man of action is free to act, so *homo faber* is free to make what he wants to; necessity enters once he has started fabrication. The *animal laborans*, on the other hand, is never free but always under the necessity to labor. Yet for man, the burden of necessity is only relieved by the freedom to choose necessity — in the activity of thinking and in creative action, the two are inseparably mixed; hence the reports that thinking, or writing a poem, are at once the most glorious and the most gruelling experiences.

It may be that the ascription of irrationality to "value" in contemporary social science, combined with the insistence on the means-end scheme, whether in the analysis of action or modified in structural-functional theory, wishes to preserve both freedom (in the irrationality of ends) and necessity (in the espousal of the means-end scheme). If so, however, the relation to both, freedom and necessity, is essentially

a relation of consumption — to have one's cake and eat it, too. Once the means-end scheme is embraced as the most conspicuous avenue for exploring the world, the sociological *homo faber*, forced as he is to espouse the idiosyncratic character of ends, transcends himself in the direction of *animal laborans*, and both meaningfulness and the very instrumentality he celebrates are in danger of being lost to the necessity of labor.

(d) *Man's Historicity and Dualism*. — In *The Human Condition*, Arendt shows in many instances how men have not so much penetrated, as been penetrated by, their time and place. The question of man's historicity — the possibility of his transcending it and the nature and extent of this possibility — has, of course, been a major preoccupation of historicism, the sociology of knowledge, and, in a certain sense, cultural relativism; but the new complexion this question takes on in Arendt's view of it gives it new cogency. This view, as we have seen, appears to be that we do not know the meaning of a story, not of part or all of man's history, until it is over — but we must add that the question when it is over is not exclusively a matter for causal analysis but also one for interpretation, through which the end of the story reveals itself to us along with the meaning of the story, as integral to this meaning (cf. *1c* above). We must add this in view of, not only Arendt's conception of action and our analysis of it, but also her discussion of the two major historical reversals she describes — the replacement of the *vita contemplative* by fabrication, that is, one of the activities (that of *homo faber*) of the *vita activa*,²⁸ and the replacement of *homo faber* by *animal laborans*. In fact, many of our questions in Section (1) above concerned the tenability of parts of her interpretation of Western man's history. Aside from these criticisms, however, to generalize from the two historical reversals as we did is another way of formulating her proposition that man is both free and not sovereign, that is, not master of the meaning of what he is free to do.

Let us add what may sound paradoxical but seems nevertheless to be the case: that man is free within the world of necessity, but non-sovereign within the world of freedom. To realize this brings us closer to answering our question about the limits of man's historicity. Man is free to deal with the world to the extent that this world is characterized by necessity and he knows this. I am free to fell a tree, provided I have all that is necessary to do so (e.g., an adequately keen axe and strong body) and provided the infinite number of causal, necessary nexuses that result in the tree and its characteristics

(e.g., its species, height, quality of timber) necessitate its falling under my axe. Furthermore, I am free to cut the tree down to the extent that the contract I made to do so is binding and thus enters the world of necessity (e.g., buying the land on which the tree stands, the purchasing contract containing no clause against tree-felling, or contracting with another person to cut the tree as a paid service or as a favor). On the other hand, I am not free to cut the tree if I do so as a poacher because then I deny the world of social necessity, whether I try to escape or to change it (though not of natural necessity, as long as my body is strong and the axe keen). To generalize, I am free to act within that part of the world of necessity in which I find myself; let us call this the "relevant world of necessity". No matter how variable this world is — my private room in which I compose while having or not having a headache; the prison from which I try to escape; the mathematical problem I am working at, the necessary premises of my thought which set limits to it — it is always characterized by necessity.

But I am not sovereign within the world of freedom, not master of the meaning of what I am free to do. The important point here is not to confuse "meaning" with "causal consequence".²⁹ For, causal consequence belongs in the world of necessity, and if I am not master of the consequences of my doing, it is only because of insufficient knowledge of this world or insufficient interest in it. "Meaning", on the other hand, belongs in the world of freedom and is uncontrollable, precisely, because not only I am free, but all men are free, within the world of necessity, that is, free, within this limit, to interpret and act on my actions in at least more than one way. For as soon as there is more than one way, necessity no longer rules alone. How many ways there are is indeed codetermined by necessity, by all the causal, necessary nexuses, as in the case of the tree, that result in the particular persons that all the interpreter-actors are, but unlike trees, persons are also characterized by freedom, the freedom to raise questions, to imagine, to explore and try.

The historical moment is the conjuncture of the worlds of necessity and freedom at a given time. Our question concerning the limits of man's historicity can now be formulated as the question of the changing, as against permanent, nature of his freedom and his nonsovereignty. Both being characteristics of man, they are, in this sense, permanent. The question, therefore, is whether the *relevant* world of necessity within which he is free and the *relevant* world of freedom

within which he is not sovereign are, in turn, permanent, or change. Once we thus formulate the question, we see immediately that the relevant world of freedom is identical with the world of freedom as such, that there is no such thing as a "relevant world of freedom". Not being a world of necessity but of meaning, it is not located in space and time, as the world of necessity is; the world of freedom which enters the conjuncture of the historical moment as one of its two components does not change. Man is not only permanently non-sovereign; the very meaning of this non-sovereignty is permanent.

In other words, man's non-sovereignty is not subject to history; only his freedom is historical. The question regarding his historicity, therefore, is limited to that concerning the historicity of his freedom. And his freedom is indeed historical: it is subject to limitations from two sources, the relevant world of necessity and the world of freedom itself.

The relevant world of necessity is a source, in turn, of two kinds of limitations, those imposed by its very nature and those imposed by my ignorance of this nature. On the one hand, coal is such that I cannot eat it; on the other hand, I may not know its combustibility. In general, whenever we speak of beating our head against a wall or use similar expressions, the chances are that we refer to our limitation owing to the nature of things; when we speak of unexpected results, side effects, unanticipated consequences, and the like, the chances are that we refer to our limitation owing to our ignorance. Our freedom changes, that is, increases, with our knowledge of the world of necessity which, in so far as it imposes limitations on us by virtue of our ignorance, decreases in relevance with our increasing knowledge. The nature of the world of necessity remains an ultimate source of limitations on our freedom, but its relevance depends on our knowledge. "*Savoir est pouvoir*" is qualified by the fact that both knowledge and strength are lodged in the world of necessity; it means "to know the necessary is to be able to find our bearings".

The world of freedom, of meaning, action, will, thought, imagination, clearly, is another source of limitations on our freedom. Examples of such limitations coming from the world of freedom are commands, intentions, decisions, arrangements made by other persons or by myself and binding on me, hindering me, restricting me; as well as my own beliefs, desires, ideologies, prejudices, etc., in short, large sections of my "culture". The difference between my freedom *vis-à-vis* the world of necessity and my freedom *vis-à-vis* the world of freedom

itself is the difference that results from the fact that the world of necessity is, precisely because it is not the world of freedom, a-human, while the world of freedom is human. Gravity I share with nature, and the necessities of my body with all organisms, but will, intent, meaning I share only with other men. *Vis-à-vis* necessity I can only accept (whether the necessary be a part of nature, like sunshine, or a man-made object, like a house), reject (escaping a thunderstorm, fleeing or fighting a political régime), or know (sunshine and thunderstorm, house and political régime). *Vis-à-vis* freedom, on the other hand, or, better, within the world of freedom, I can talk, think, act, persuade and be persuaded. Action and speech are closely akin, and both are located in freedom, as we have already been told by Arendt.

Necessity and freedom, however, are mixed because man lives in the world of both, although, to repeat, that which is exclusively human is only the latter. Determinisms of all kinds stress necessity or even deny freedom outright; philosophies of freedom and some philosophies of life emphasize freedom. Whereas it seems incontrovertibly true that man is both free and limited by necessity — no matter how his situation be formulated, and surely it can be formulated better than it has here been — the actual mixture of the two elements is subject to investigation in respect to individuals, groups, societies, times. We have many proclamations on the subject, but no scientific discipline that is devoted to it, not even a program for such a discipline. There are, on the other hand, myriad investigations in all disciplines, in the natural and social sciences and in the humanities, which the enterprise envisaged could draw on; particularly important among them are efforts made in the “sociology of knowledge”.³⁰

To conclude, man’s historicity is the changeability inherent in his freedom which, with his unchangeable non-sovereignty, makes up his dual nature. Changeability inheres in his freedom because he lives in two worlds, those of necessity and freedom; and he is free to reduce the limitations flowing from these two worlds through, respectively, knowledge, acceptance, rejection; and thought, speech, action. How he has done so in the past and may perhaps in the future, and hence what the nature of the relations between the two components of man and their relative weight, is the subject matter of a not yet existing science.

(e) *Conclusion.* — Our analysis of Arendt’s *The Human Condition* has urged revisions that we must make as social scientists if we would clarify our practice, our place in the world, our attitude toward it,

the nature of our chances to act in it — just as the failure to make such revisions would muddle our practice, giving theoretical sanction to our world alienation, to our point in history, exhibiting our time that we may have thought we were analyzing, and evincing, unknown to us, our loss of a common human world. The revisions increase our freedom, shrinking the world of necessity relevant to us.

In line with our distinction of theory and practice which we stated at the beginning of our venture and have maintained since (and which, it may be seen, is related to that of freedom and nonsovereignty), we arranged our dealing with Arendt's book in a theoretical and a practical part. Theoretically, what we must do is to find out what is the case. In the part on theory, we tried to signal some things, at least, that need finding out. Many of these, to be sure, were matters of conceptual clarification, but some of them, as well as what we said at the very end of the part on practice just concluded, need little developing to appear in the form of areas (vast areas, to be sure) of research. I shall merely list these, in recapitulation, as the terminal point, for the time being, of our inquiry occasioned by Arendt's book.

(1) Behavior in past societies and in contemporary society.

(2) Past and contemporary societies analyzed in terms other than behavior.

(3) History of the term "value" and its colloquial and academic uses.

(4) Analysis of cybernetics, game theory, information and communication theory, operations research, and similar developments in contemporary social science in respect to elements of *homo faber* and *animal laborans* operative in them.

(5) The nature and historically changing relative weights of freedom and necessity in man (this suggests the beginnings of a new science).

NOTES

- ¹ Hannah Arendt, *The Human Condition*, Chicago: University of Chicago Press, 1958. Figures in parentheses refer to pages of this book. An earlier version of this paper was developed in connection with a seminar on "The Means-End Scheme in Contemporary Sociology and Its Relation to an Analysis of Non-violence", conducted at the Institute for Social Research, Oslo, August, 1959; for helpful suggestions, I am indebted to members of this seminar, quite particularly to Gene Sharp. An earlier version of Part One was presented at the annual meeting of the American Catholic Sociological Society, Notre Dame University, December, 1958.
- ² Personal communication from Dr. Arendt. *Vita Activa* is the title of the German edition of the book (Stuttgart: Kohlhammer, 1960).
- ³ In support of this, Arendt discusses the Latin, English, Greek, French, and German words for "labor" (48, n. 39; cf. 80, 81, n. 5, 101).
- ⁴ The beginning of such an examination has been made, e.g., by Ralph Ross and John W. Bennett in their reviews of the book in, respectively, *The New Leader*, 29 September 1958, and *American Anthropologist*, August, 1959.
- ⁵ Arendt herself illustrates this with the case of the hermit (22).
- ⁶ The difficulties inherent in the logic of ideal-types have been well known, at least since Max Weber. See a recent essay, with bibliography, by Don Martindale: "Sociological Theory and the Ideal Type", in Llewellyn Gross, ed., *Symposium on Sociological Theory*, Evanston, Ill.: Row, Peterson and Co., 1959, pp. 57-91.
- ⁷ Talcott Parsons, *The Structure of Social Action*, New York: McGraw-Hill Book Co., 1937, pp. 733, 740 (*Structure*).
- ⁸ On Arendt's conception of "miracle" (*"Wunder"*), see her "Freiheit und Politik", *Die neue Rundschau*, 69, 4 (1958), pp. 1-25, esp. 20-22.
- ⁹ And more centrally and in more detailed and explicit argument in her article on freedom and politics, *ibid.*
- ¹⁰ As in the remarks on the etymologies of *zöon politikon* vs. *animal socialis* (23), "society" (23, n. 3), "labor" (referred to before), *polis*-*"law"*-city (63-64 and n. 65), *homo faber* (136 and n. 1), "object" (137, n. 2), "nature" (150), "action-beginning" (referred to before), and "power" (200).
- ¹¹ Hannah Arendt, "Reflections on Little Rock," *Dissent*, 6 (Winter, 1959), p. 54.
- ¹² *Ibid.*, pp. 54-56.
- ¹³ Max Weber, *Wirtschaft und Gesellschaft*, Tübingen: J. C. B. Mohr (Paul Siebeck), 1925, p. 1 (*WuG*); cf. *The Theory of Social and Economic Organization*, transl. by A. M. Henderson and Talcott Parsons, ed. with an introduction by Talcott Parsons, New York: Oxford University Press, 1947, p. 88 (*Theory*).
- ¹⁴ Space limitations prevent me from doing more in support of this view than to refer to a paper in which I attempt to argue it: "The Sociology of Knowledge and Sociological Theory", in Llewellyn Gross, ed., *op. cit.*, pp. 567-602.
- ¹⁵ Cf. *WuG*, pp. 12-13; *Theory*, pp. 116-18.
- ¹⁶ Developed in his comments on "understanding"; cf. *WuG* p. 4; *Theory*, p. 96.
- ¹⁷ The second point similarly applies to the question whether or not an event constitutes "change". The birth of a child is almost certain to be thus considered by the members of the child's immediate family; but it is not if, instead of the family, their country is the social unit in reference to which the event is assessed.

- ¹⁸ It should also be noted that "*Wertbeziehung*" and causal efficacy — the two predicates either of which determines a "historical unit" for Max Weber, are intimately related to our, respectively, "meaningfulness" and "consequences". Cf. Weber, "Critical Studies in the Logic of the Cultural Sciences" (1905), in *On the Methodology of the Social Sciences*, transl. and ed. by Edward A. Shils and Henry A. Finch, Glencoe, Ill.: Free Press, 1949, esp. p. 159 (*Methodology*).
- ¹⁹ Cf., e.g., "Ray Lepley (ed.), *Values: A Cooperative Inquiry* (New York: Columbia University Press, 1949), which contains the essays of thirteen philosophers . . . A reading of this volume pointedly shows the divergence of opinion even among scholars of somewhat the same school of thought. The editor remarks, with a note of defeat, in his Preface: 'At one stage of the inquiry it was hoped that agreement might be reached upon a common glossary of fundamental value terms . . .'" (Louis A. Ryan, O. P., "Value Judgments in Selected American Introductory Sociology Textbooks, 1947-1950," Ph. D. dissertation, Ohio State University, 1957, p. 11, n. 5).
- ²⁰ Abraham Edel, "The Concept of Levels in Social Theory," in Llewellyn Gross, ed., *op. cit.*, p. 189 (paraphrasing R. B. Perry).
- ²¹ Clyde Kluckhohn and others, "Values and Value-Orientations in the Theory of Action", in Talcott Parsons and Edward A. Shils, eds., *Toward a General Theory of Action*, Cambridge: Harvard University Press, 1951, p. 395.
- ²² Max Weber, "Science as a Vocation" (1918), in *From Max Weber: Essays in Sociology*, transl., ed. and with an introduction by H. H. Gerth and C. Wright Mills, London: Kegan Paul, Trench, Trubner, 1947, p. 151.
- ²³ Max Weber, "The Meaning of 'Ethical Neutrality' in Sociology and Economics" (1913-17), *Methodology*, p. 38.
- ²⁴ Cf. *ibid.*, pp. 22-27.
- ²⁵ See also *c* below.
- ²⁶ More recent developments in social science, especially cybernetics, game theory, information and communication theory, operations research, etc., need study in this connection. Note such pivotal terms as "input-output", "feedback", "strategies", and others, all of them taken from economics and engineering, the realm of various types of *homo faber*.
- ²⁷ For minimal references, cf. *WuG*, pp. 12-13 (*Theory*, pp. 116-18); *Structure*, pp. 648, 733; for a less technical textbook presentation, see Kingsley Davis, *Human Society*, New York: Macmillan Co., 1949, pp. 120-46.
- ²⁸ We have not discussed this first reversal in this paper; on the history of contemplation, consult *The Human Condition*, Index under "Contemplation", and esp. pp. 14-17, 20-21, 291, 301-02.
- ²⁹ This is a distinction Weber took great pains to clarify; cf. *Methodology*, esp. p. 158.
- ³⁰ Cf. Kurt H. Wolff, "A Preliminary Inquiry into the Sociology of Knowledge from the Standpoint of the Study of Man", *Scritti di Sociologia e politica in onore di Luigi Sturzo*, Bologna: Nicola Zanichelli, 1953, Vol. III, pp. 585-618; *The Sociology of Knowledge and Sociological Theory*, *loc. cit.*

MEANING, REFERRING, AND THE PROBLEM OF UNIVERSALS

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The problem of universals, at least in its modern form, often begins from questions which seem, in principle, decidable by the sorts of experimental procedures carried on in descriptive semantics, or in applied linguistics. These are questions about the role which pronouns, common nouns, adjectives etc. play in natural languages. But these apparently scientific questions are interpreted by philosophers in ways which give rise to metaphysical conundrums — to problems which are not in principle decidable. The paper traces some of the factors which impel philosophers to interpret these questions in the way they do. The author's thesis is that questions about the roles which linguistic expressions play are often interpreted as questions about the meaning of these words, and these, in turn, are thought to be questions asking for the identification of differing sorts of objects in the universe (e. g., particulars, universals). The author attempts to show in detail why such interpretations of ordinary questions are improper.

INTRODUCTION

The problem of universals in its modern guise typically begins with the task of explaining the difference between the ordinary uses of common nouns and certain adjectives on the one hand, and of proper nouns on the other. It reaches fruition when, supposedly in the course of giving such an explanation, perplexities arise about the nature of the ingredients composing the universe. These latter difficulties, sometimes labelled "ontological" and traditionally taken as forming the heart of the problem, express themselves in such apparent requests for information as "Are there universals?", "Where do universals exist?," "Do universals exist in time or out of it?," and so forth through a familiar gamut. Philosophers have seldom explicitly distinguished the linguistic task from the ontological one¹, a lacuna which in its own right is a matter of some interest since the two seem *prima facie* so disparate and so susceptible to different treatment. For detached from each other, and considered independently, the former seems a paradigm of a descriptive

problem in applied linguistics, and open to an objective solution, at least in principle; while the latter looms as a metaphysical conundrum *par excellence*, a veritable maze of knots and tangles with no paths to daylight. It is thus a matter of considerable curiosity, and one worth investigating, that philosophers have often slipped so easily from doing the one to doing the other. I propose therefore in this paper to lay bare at least some of the factors which are, or which may be, responsible for such a move. In doing this, I hope to show that this move is unnecessary, at least insofar as it is occasioned by the effort to give a correct account of the workings of language.

I

The slide from trying to give a true account of the employment of common nouns, adjectives and proper nouns in everyday discourse frequently commences when philosophers interpret this task as demanding an inquiry into the meaning of these types of words. To ask, "What do common nouns mean?" or "What do adjectives mean?" are questions which seem natural and innocuous. These questions apparently differ only in generality from questions we may in fact pose about the meaning of particular common nouns, such as "What does 'puce' mean?" or "What does 'serendipity' mean?" and so on. It is natural for the philosopher, given as he is to generalizing, to extend the scope of such specific inquiries into broader ones about the meaning of all expressions of a similar type; but it is not innocuous. For a general question of this sort has no answer in the way in which particular questions do, and any attempt to excogitate one is merely confounding. Suppose after spelling your name for a foreigner, he were to ask "How do Americans spell their names?", how would one reply? If there is an appropriate response to such a remark, it would seem to be "Well, it all depends upon the name in question." So it is with the common noun in question — what it will mean will depend at least in part upon what noun it is, and hence no general answer to such a general question is appropriate. Unquestionably, part of the impetus toward ontology stems from the attempt to treat questions about the meaning of types of words as if they were on a par with questions about the meaning of particular words, a procedure which might best be characterized as a category mistake.

But this is not the sole mistake which the ontologist is prone to make when he deals with the problem of universals. Even beyond this, he

often assumes as true a certain conception of meaning which he imports into his formulation of the problem and to which he expects his answers to conform. Strawson has devastatingly criticized this conception in his paper "On Referring," (*Mind*, April, 1950), pointing out that, in effect, it identifies the meaning of at least certain types of words with the object or objects that these types of words "stand for" or "designate," or as Strawson has put it, "are used to refer to." Russell's writings, whether discussing the problem of universals or other topics, from his earliest works to the present, are replete with statements illustrating his adherence to this doctrine. In the *Introduction to Mathematical Philosophy*, p. 175, he says:

We have, then, two things to compare: (1) a *name*, which is a simple symbol, directly designating an individual which is its meaning and having this meaning in its own right, independently of the meanings of all other words; (2) a *description* which consists of several words, whose meanings are already fixed, and from which results whatever is to be taken as the "meaning" of the description.

Or again, in *The Problems of Philosophy*, p. 104, specifically discussing the problem of universals, he says:

One way of discovering what a proposition deals with is to ask ourselves what words we must understand — in other words, what objects we must be acquainted with — in order to see what the proposition means.

Russell imports this conception of meaning into his formulation of the problem of universals. In *A History of Western Philosophy*, he presents the problem as follows:

Plato's doctrine of ideas contains a number of obvious errors. But in spite of these it marks a very important advance in philosophy, since it is the first theory to emphasize the problem of universals which, in varying forms, has persisted to the present day. Beginnings are apt to be crude, but their originality should not be overlooked on this account. Something remains of what Plato had to say, even after necessary corrections have been made. The absolute minimum of what remains even in the view of those most hostile to Plato, is this: that we cannot express ourselves in a language composed wholly of proper names, but must have also general words such as "man," "dog," "cat"; or, if not these, then relational words such as "similar," "before," and so on. Such words are not meaningless noises, and it is difficult to see how they can have meaning if the world consists entirely of particular things, such as are designated by proper names. There

may be ways of getting around this argument, but at any rate it affords a *prima facie* case in favour of universals. I shall provisionally accept it as in some degree valid.²

Admittedly one may misrepresent an author by selecting quotations from various works composed by him over an extended period. The danger is especially great in dealing with someone like Russell who has written voluminously on many topics, about which he has changed his mind from time to time. But I submit that these quotations are faithful in at least two respects. They exhibit a theory of meaning which, so far as I know, he has never entirely abandoned in spite of occasional castings in other directions; and likewise, they contain the elements of an argument which, in one form or another, he has proposed over and over again to demonstrate the existence of universals. Let me refer to it henceforth as Russell's argument, and in order more clearly to exhibit the role which his theory of meaning plays in it, I shall restate it as follows:

1. All words having independent meaning stand for (or mean) the objects they designate (or name).
2. Proper names are independently meaningful and thus designate certain objects.
3. These objects are individual things (particulars).
4. General and relational words are also independently meaningful and thus also designate certain objects.
5. These objects are not individual things but general things (universals).
6. Hence, universals exist.

The quotations cited do not do justice, of course, to the many subtleties contained in Russell's extensive discussions of the problem of universals. But they are useful in a number of respects. To begin with, they show that Russell's path into the problem is via linguistic considerations; he is concerned to give an account of the different roles played in everyday speech by common nouns, proper nouns and relational terms. The nub of his account is that the role which general words play is similar to that which proper nouns play. Both types of expressions are used "designatively," i.e., to refer to, or stand for, certain features of the world. Russell is able through this explanation to indicate why we need both general and singular terms in any adequate explanation of the world. For the world contains sundry objects,

particulars and universals at least, and diverse types of words are needed to mark them off from one another in our talk about them. Secondly, the quotations show how Russell brings his conception of meaning to bear upon the problem, not only by asking general questions about the meaning of various types of words, but also by regarding the problem of giving an account of the correct use of these types of words as being an investigation into their meaning. And, finally, when Russell says, "Such words are not meaningless noises and it is difficult to see how they can have meaning if the world consists entirely of particular things, such as are designated by proper names," it seems a fair interpretation that the difficulty which he mentions in this passage is due to his identifying meaning with designating. This identification causes him to conceive the task of discovering how general words can be significant to be the task of locating certain general objects in the universe which can serve as the designata of such words. That Russell has not abandoned this point of view is evidenced by his recent comments in *Mind* (July 1957, P. 387) where he writes, "I defy Mr. Strawson to give the usual meaning to the word 'red' unless there is something which the word designates."

The above remarks may be taken as initial steps toward illustrating how linguistic considerations may lead a theorist by natural moves into ontological puzzles. Although the path traced thus far is one which is commonly followed, it is not the sole route into perplexity. As I shall indicate in the next two sections, even if one rejects Russell's conception of meaning when one sets out to explain how language works, it is still possible, again by a series of natural moves, to become enmired in ontological questions. I can best show this, I think, by illustrating how alternative conceptions of meaning proposed by Quine and Strawson, respectively, do not overcome the lures which beckon towards the ontological trap.

II

Quine conceives³ the linguistic task to be one of showing how general words can be significant even if they do not "name" anything. In this respect, his route into a discussion of the ontological issues is not unlike that we have already adumbrated, although it should be stressed that the story would not be complete without a discussion of what Quine takes the relation to be between quantification theory in logic and commitment to a specific ontology⁴. Fascinating though this story may be, it is a digression in the tale we are telling here, since we wish to

show at this stage only that even should we adopt Quine's proposal to distinguish between meaning and naming, we cannot easily quell the impulses to engage in ontological speculation.

Indeed, Quine commits himself to a certain ontological position, a sophisticated variation upon traditional nominalism, as a result of his investigations. But he is not attracted to his ontology by accepting Russell's theory of meaning. In fact, the burden of his paper is to show that one may reject the arguments in favor of Russellian realism since these arguments depend for their cogency upon an untenable conception of meaning. The untenable conception fails to discriminate between meaning and, as Quine terms it, "naming." At this point, a pair of clarificatory asides may be useful. By "naming" Quine does not mean what is, in my view, a basic common use of that word, i.e., in such sentences as "We named him 'John' at the Christening". In this use "naming" is roughly to be understood as "giving a name to." Instead, Quine employs it as it is used in sentences like "John only named three of the shrines he visited, but there were doubtless more" where it is clear that what is intended is to be rendered as "John only referred to (mentioned, identified) three of the shrines by name." The other point which should be noted here is that Quine *roughly* means by "naming" what Strawson means by "referring", although with at least one outstanding difference. Strawson emphasizes that referring is a characteristic of a use of an expression, while Quine talks as if it were a characteristic of the expression itself. This difference has important consequences in that it produces an inclination in Quine to ask general questions about the meaning of certain types of linguistic expressions. But to return to the main thread: Quine's position is that, on the basis of arguments proposed by Frege, a distinction must be recognized between meaning and naming — as he says, "there is a gulf between *meaning* and *naming* even in the case of a singular term which is genuinely a name of an object." Once it is seen that meaning and naming are dissimilar, then it is clear that general terms may have meaning (or be significant) even though they name nothing. Since Russell's argument in favor of universals had presupposed that in order to be significant such words must name, this argument can be rejected.

Quine is surely correct both in urging that meaning must be distinguished from naming (in his sense) and also in holding that the distinction disposes of the realist position insofar as it takes the form we have considered. But even if we grant the merit of Quine's distinction we still have not disposed of the urge to go from linguistic considerations

into ontology — indeed, as I will now indicate, even into a realist ontology, let alone a nominalistic one. For Quine seems to accept the position that when a word is a genuine name it must be the name of something. The purpose of his argument against the realist is to show that common nouns are *not* names (and then further to show how they can be significant even though this is not so). But it is clear that Quine believes that if they were names, or perhaps genuine names to be more explicit, they could be so only if the world contained entities which they in fact did name. So the argument becomes destructive of the realist position only if it is true that common nouns are not names. And how does Quine establish this? He tries to do so by attributing to the realist the view that such predicates as “red” or “is red” (see p. 198 of “On What There Is”) must be names in order to be meaningful at all. Although this may be true of Russell’s version of realism, it is not essential to the realist position. All the realist has to maintain is that common nouns are names; he does not have to maintain the further thesis that they must be names in order to be meaningful. If he can establish that common nouns are names, he may appeal to Quine’s own principle and argue that if they are names they must name something — and then submit, as a way of accounting for the difference between them and proper names, that what they name are universals.

Now it seems evident that common language in this matter supports the realist against Quine; for users of ordinary speech do regard common nouns as names. Do we not say “‘Red’ is the name of a color”, or again, “He named three colors, ‘scarlet,’ ‘carmine,’ and ‘magenta,’” and so on. If it be granted that common nouns are names by such appeals to our normal speech habits, then the realist move from these linguistic considerations into his ontology seems natural indeed. He can reconstitute his position without ever mentioning meaning at all. He can raise the ontological issues by framing them as questions about names, e.g., “What do common nouns name as distinct from what proper nouns name?” and so forth. Quine-ine therapy, strong though it may be, does not dispose of the realists’ ontological fevers.

III

In “On Referring” Strawson has sketched what seems to me the best picture yet produced of how ordinary speakers of English employ common nouns, adjectives and proper nouns⁵. In the course of doing

this, he has developed a pair of distinctions, the first between meaning and referring; the second between referring and ascribing. Since Strawson's discussion of these matters is carefully qualified, and moreover, by now familiar to most analytical philosophers, I shall not bother to summarize his results, but instead proceed to show how they also fail to relieve the pressures which push the philosopher into ontological difficulties.

Although Strawson, so far as I know, has not drawn any inferences about the sorts of entities composing the universe from his investigations into language, the distinctions he has proposed are applicable to the problem of universals. To begin with, Russell's argument in favor of realism can quickly be disposed of since it depends upon an identification of meaning and referring. Secondly, the distinction between referring and ascribing can be used to attack the realist defense against Quine. As I pointed out above, this defense consists in holding, by reference to ordinary speech, that common nouns are names, and if names, then the names of something, presumably universals. In showing that common nouns and adjectives when used as grammatical predicates are generally employed in order to ascribe a characteristic to whatever the sentence that contains them is about, Strawson would (had he considered the matter) explicitly deny that these terms are used as names *in Quine's sense* i.e., for purposes of referring. Therefore, the realist is not entitled to claim that in such sentences as "Socrates was snub-nosed" both the word "Socrates" and the word "snub-nosed" are employed to refer to ingredients of the universe; for the latter at least makes no reference to anything, but instead is utilized to ascribe a characteristic to Socrates.

Nonetheless, the ontologist is not so easily dismissed, and merely regards this argument as a gambit in a game which two can play. For even if he accepts both that there is a distinction between meaning and referring, and also between referring and ascribing, he has at least two further ways of raising the traditional ontological problems while employing this very terminology. He may say, to begin with, that when we assert "Socrates was snub-nosed" we are making a true statement about Socrates, and in the course of doing so, we can truly be said to be ascribing a characteristic to him. But if so, there must be such characteristics (or universals), and whether they are named by predicates, or ascribed by predicates, is a terminological finesse which misses the main point. He can fire one more shot as well. For, as Strawson himself has pointed out, when common nouns are employed as the

subjects of sentences, they may well be used for purposes of referring to something, although not necessarily for purposes of making unique references (see Part IV of "On Referring.") Thus, the realist is entitled to ask, "What is it that one is referring to when he utters true sentences of the form 'Red is a color'; or 'Grease is slippery', by such words as 'red' and 'grease'?" Presumably, these words are not used to refer to particulars since we are not merely speaking about this red thing or that piece of grease; but if not to particulars, then why not to universals?

IV

Not all the pressures which motivate the ontologist can be attributed to his misconceptions about the working of language, although unquestionably some can. Sometimes the causes go deeper. Models of the world which he creates can originate in sundry ways. It may be that when the ontologist strains the bonds of everyday speech, his misconceptions about and misuses of language may not be the causes of metaphysics but rather symptoms of underlying causes. Nevertheless, what is peculiar about such symptoms is that they are often susceptible to therapy in much the same way as the causes themselves. One who attends to the way language is in fact employed may find the inclination to express his perplexities about the nature of the universe etiolated, and in the end, wonder perhaps if he need have concocted them at all. As Bentham tells us when he found himself "unexpectedly entangled in an unsuspected corner of the metaphysical maze,"

A suspension, at first not apprehended to be more than a temporary one, necessarily ensued: suspension brought on coolness, and coolness, aided by other concurrent causes, ripened into disgust.⁶

With some such therapeutic aim in mind, we shall now try to show in detail how such misuses and misconceptions take place.

Let us begin with questions which the ontologist may ask about the *meaning* of certain words and reserve a discussion of referring until later. I have already pointed out the artificiality of the questions some philosophers ask about the meaning of all words of a certain grammatical type, and the difficulties which ensue when they attempt to construct answers to such queries. In the ordinary employment of language, whenever one asks a question about the meaning of a word, or group of words, one is generally asking about the meaning of some

particular word or group of words within a given grammatical type. But even in these cases the ontologist can misconstrue answers which are appropriate as I now propose to show.

Let us begin by inquiring under what conditions one is likely to ask "What does the word (where one has mentioned a particular word) mean?" To give an exhaustive list of all the circumstances in which such a sentence might be uttered would not be relevant here, even if it were possible; we are interested primarily in the standard uses of such sentences so that we can see how philosophical uses diverge from them. (Among the irrelevant uses might be such queries as "What does the word 'love' mean to him?" said by a woman spurned, or even "What does 'Sarah' mean?" where the answer given, after consulting a dictionary, is "princess", and so forth). A few examples can illustrate the standard cases. We may expect a foreigner or a child to ask such a question. He will have heard a word mentioned, or read it somewhere, and does not know what it means. Suppose the word is "fustian". He may ask "What does 'fustian' mean?" and we may properly reply by citing the dictionary definition. Even if a person is a native speaker of a language, he may find himself in circumstances where such a question becomes relevant. These will be where he hears or reads a word and does not know what it means. This is often the case where a word is rare, or is a technical term like "duodenum". But once again an appropriate reply is at hand in the form of a dictionary definition.

I have cited these cases, which are standard ones, because they illustrate that the kinds of questions we can expect people to ask will vary depending upon how much knowledge they possess. For instance, if a foreigner or a child hears a proper noun, it is more reasonable to expect that he will not know that it is a proper noun than to expect an adult, native speaker not to know this. When a foreigner, or a child, or perhaps rarely an adult native speaker, hears a proper noun and does not know that it is one, for instance "Sacramento", he may ask "What is the meaning of 'Sacramento'?" But such cases are exceptional. Generally, he will be able to infer from the linguistic context itself that the word is a proper noun, or in some other way excogitate this fact. When he knows that such is the case, he will normally not go on to ask what the word means. In circumstances where he does not know, or cannot figure out, that a word is a proper noun and asks a question about its meaning, for instance, "What does 'Jones' mean?" the oddness of the question strikes us at once. The

proper response in such circumstances is to inform him that the word means nothing, that it is a proper name (we are, of course, excluding such cases as "Sarah" means "princess".) One may wish to elaborate this answer and say "Jones is a philosopher, you know," but it is clear that this utterance forestalls the further question, namely "What is Jones?", a question the speaker might reasonably be expected to go on to ask once he is informed that 'Jones' is a proper name. But "What is Jones?" is clearly different from the original question "What does 'Jones' mean?", since by hypothesis in asking this latter question the speaker did not know that 'Jones' was a proper name at all.

One of the most important moves toward developing the ontological perplexities involved in the problem of universals can occur at this juncture. The philosopher recognizes that in ordinary speech we do ask questions about the meaning of words, but he overlooks the fact that in standard situations the words asked about are generally *common* nouns (the exceptions occur as we have said when the speaker does not recognize that the word is a common noun). Since such questions about the meaning of a particular common noun are standard in ordinary English, and since they may also sometimes (but inadvertently) be asked about particular proper nouns, the philosopher tends to think that questions about the meaning of proper nouns are also standard and in no way unusual. However, as soon as he asks such a question about the meaning of a particular proper noun he sees that it has no answer in the way in which "What does 'fustian' mean?" has an answer. Puzzled, because he thinks the question "What is the meaning of 'Jones'?" to be an appropriate one and because he cannot find an answer to it, the philosopher is forced to invent one⁸. Typically, the answer he invents is "This is" said while pointing to Jones, if Jones happens to be available. But such a response is inappropriate as we have shown, since it indicates a misunderstanding of the kind of circumstances in which a question like "What does 'Jones' mean?" might be asked, i.e., when the speaker does not know that "Jones" is a proper noun.

Why the philosopher should choose "This is," as a paradigm of an appropriate response to the question "What is the meaning of 'Jones'?" instead of doing what would normally be done in such a case, i.e., explaining to the speaker that it is a proper name and has no meaning, is an interesting question. I submit that the explanation lies in his confusing questions about the meaning of a particular word with questions calling for the identification of something. Let me

explain this in greater detail. The sentence "This is," said while pointing to Jones, is presumably an abbreviation of a fuller answer to the original question "What is the meaning of the word 'Jones'". The fuller answer, strictly speaking, should have been "This is the meaning of the word 'Jones'", said while pointing to Jones. But if this answer were in fact ever given it would strike the listener as very odd; indeed as senseless. It strikes the philosopher as odd, too; but instead of conceding its oddness and then searching for the reasons which led him to frame such a peculiar answer, he tends to minimize its peculiarity by assimilating it to answers which are not odd. And this is the role which "This is Jones" plays. "This is Jones" is a perfectly proper response to the perfectly ordinary question "Which of them is Jones?" asked in circumstances where the speaker wishes Jones to be identified. The two answers do not seem very different to the philosopher, since they both shorten to "This is." But when the philosopher so shortens "This is the meaning of 'Jones'" it becomes identified in his thinking with "This is Jones", and assumes the protective coloration of that plain reply to a plain question. In this way, he camouflages the strangeness of his original answer to his equally unusual original question "What is the meaning of the word 'Jones'". But in adopting "This is Jones" as his model of a proper reply he also assumes that the question "What is the meaning of the word 'Jones' " calls for an identificatory reply, like "This is Jones". From this position, by generalizing, he is led to a conception of meaning which holds that the task of giving the meaning of all words of a certain grammatical type is identical with the task of identifying the objects they are used to "stand for" or "refer to", or however he wishes to put it.

The road to perplexity is now widened. For once he is convinced that "This is", said while pointing to Jones, is an appropriate answer to the question "What is the meaning of 'Jones'?" then he is prone to extend this type of answer to questions about the meaning of common nouns, ignoring when he does so the standard type of replies which are normally given to such questions, such as those we have cited. Once again, he interprets a question about the meaning of, say, the word "human" as calling for an answer identifying something. But this is where his problems begin in earnest, since there is nothing he can point to when he says "This is human", which differs from what he can point to when he says "This is Jones". So perplexed, he commences to talk about the difference between common characteristics and their exemplifications, and soon is deeply enmeshed.

In sketching these moves into philosophical bewilderment, I have intimated that the appropriate response to one who asks for the meaning of a particular common noun like "serendipity" is always to cite an answer which is, or approximates to, a dictionary definition. This statement may be qualified in certain respects, but not essentially. It must be noted that sentences may be employed in the rich variety of everyday talk for a variety of purposes. In particular, interrogative sentences, even those containing exactly the same words, may be employed to ask different questions. What I am maintaining here is that when a *sentence* like "What is the meaning of 'serendipity'?" is used to ask a *question about the meaning* of a particular common noun, then the appropriate response is, as I have indicated, to cite either the dictionary definition, or something approximating it. This may not seem so for all common nouns, especially those which philosophers have traditionally maintained to be indefinable. But it is so. The tradition has merely served to mislead us. For suppose we are asked "What is the meaning of the word 'red'?" A proper reply consists in quoting the dictionary definition (or an approximation), i.e., "it is any of several colors the hue of which ranges from that of blood to that of the ruby or many roses." But it never consists in saying "This is the meaning of the word 'red'" while pointing to a rose. Such an utterance in ordinary situations would be as odd as "This is the meaning of the word 'Jones'" said while pointing to Jones. Accordingly, such an answer must be distinguished from an answer like "This is red," which is an appropriate, identificatory response to the question "Which of them is red?" but that this latter question is wholly distinct from one about the meaning of 'red' is, on the basis of the foregoing, now clear, I think.

V

Similar misconstructions are put upon ordinary uses of the word "refer". In formulating the ontological difficulties in the problem of universals, a philosopher may begin with such sentences as "To whom does the word 'Jones' refer?" and "To what does the word 'human' refer?" The ontologist thinks that an example of a proper reply to the former question will be "This is to whom the word 'Jones' refers," said while pointing to Jones, and then by analogy, argues that a similar assertion will be apposite in the case of "human" as well. At this juncture the well-known difficulties crop up. When he wishes

to say "This is what the word 'human' refers to," he finds he cannot point to anything different from what he points to when he says "This is to whom the word 'Jones' refers." If he thinks these difficulties insurmountable, he will be content to argue only for the reality of things like Jones and adopt nominalism as his creed; or if he believes they can be overcome by distinguishing between "common characteristics and their exemplifications," he will favor realism, as Russell does.

This way into ontology rests upon an elementary mistake. For as is plain, words do not refer to anything, but are used by someone to refer to something or someone. When this point is recognized, one may respond to such queries as "To whom does the word 'Jones' refer?" and "To what does the word 'human' refer?" by saying "no one" and "nothing", respectively — on the grounds that words do not refer to anything.

But although this reply is essentially correct, it is far too simple; even paradoxical. For it gives the ontologist the opportunity to re-frame his questions so as to include a reference to the user of words. He may now say "To whom did *he* refer by the word 'Jones'?" and "To what did *he* refer by the word 'human'?" and so forth. To meet this stratagem, then, other measures are required. Once again, this can be provided by describing how the ontologist deviates from standard uses of the word "refer".⁹

In one of its standard uses, "refer" occurs in questions which are designed to elicit answers identifying or mentioning someone or something. For instance, one might ask "Which Jones was it you referred to?" to which a pertinent reply might be "The philosopher", or even "This man here", said while pointing to someone. Such questions can contain common nouns as well as proper nouns, i.e., we might say "Which color were you referring to yesterday when we discussed painting the kitchen?" to which, again, a proper reply would be one identifying the color in question (e.g., "This one here", said while pointing to a swatch). Now what is significant about these questions is that the word "word" is never *mentioned* in them. And this is the standard situation when we ask a question calling for an identificatory answer. When ordinary speakers of English frame identificatory questions containing the word "refer" (or some of its tense variants) the word "word" is normally not mentioned in these questions. We do not say "To whom does the *word* 'Jones' refer?" or "To what does the *word* 'red' refer?" The queerness of these questions

can be seen by comparing them with the standard examples we have cited.

The error which the ontologist commits can now be specified. He formulates questions containing "refer" which are about words, but does not confine them to the circumstances in which questions about words are normally asked. Instead, he treats them as questions calling for the identification of someone or something. But as we have indicated, when questions calling for the identification of someone or something are normally asked, they do not mention words at all. The ontologist is forced into asking such inappropriate questions because the standard ways of calling for identificatory replies have standard, plain answers which do not give rise to ontological difficulties. By confusing questions appropriate to one set of circumstances with those appropriate to another, he initiates the traditional riddles which form the problem of universals.

What emerges from this lengthy, if sketchy, account are pictures of how some philosophers slide into metaphysics by misusing and misconstruing our ordinary ways of talking. Such slides may, of course, take place for reasons other than those we have indicated, and the pictures are far from complete. But even these pictures should give us a clue to a remedy. For as Wisdom has said, "the treatment is the diagnosis, and the diagnosis is the description of the symptoms."

¹ R. B. Brandt has recently called attention to the need to make this distinction in "The Languages of Realism and Nominalism," in *Philosophy and Phenomenological Research*, June 1957, p. 517.

² pp. 126—7 of the Simon and Schuster edition.

³ In "On What There Is," reprinted in *Semantics and the Philosophy of Language*, ed. by L. Linsky, U. of Illinois Press, 1952.

⁴ Recently discussed by Strawson in a symposium in *Philosophy and Phenomenological Research*, June 1957, entitled "Logical Subjects and Physical Objects."

⁵ His performance in the Symposium in *Philosophy and Phenomenological Research*, June 1957, is also enlightening with regard to these matters.

⁶ Preface to *An Introduction to the Principles of Morals and Legislation*.

⁷ I am indebted to Professor Peter Remnant for this example, as well as for other critical remarks on this paper.

⁸ For a different point of view see Morris Lazerowitz' suggestive article "The Existence of Universals," *Mind*, 1946, pp. 1—25. Lazerowitz contends that when the philosopher engages in ontological speculation he is in effect recommending new ways of talking about the world; this may be so (although I have reservations about the accuracy of this account) but it seems to me clear that initially, at least,

the philosopher is trying to find answers to questions which he finds puzzling. My thesis in this paper is that he need never have formulated the questions to begin with had he not misconstrued our ordinary ways of talking.

- ° Some comments about the notion of a standard use may be in order. To begin with, words may have more than one standard use, e.g., "He referred the matter to the committee for study," "Who was he referring to?" and so forth. Secondly, the antonym of "Standard use" is to be taken to be "improper use," rather than "unusual use." A use of language may be unusual in the sense that the occasion for its use may not frequently arise in everyday life; but it may be a standard use in those circumstances. We wish to show that the ontologist is guilty of an improper use of "refer", in the sense that he formulates questions appropriate to one standard use of "refer" but applies them in circumstances appropriate to a different standard use of that term.

REMARKS ON THE ANCIENT DISTINCTION BETWEEN BODILY AND MENTAL PLEASURES

by

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The author tries to show that the old distinction between bodily and mental pains and pleasures, still maintained by many ethical writers, deserves to be forgotten. An analysis of the possible interpretations of this distinction leads to the opinion that people call mental those pleasures which they have in high esteem and that they treat as bodily pleasures the ones less approved. Thus the distinction which was expected to contribute to an elaboration of a hierarchy of values already implies one, and the statement that mental pleasures belong to a higher order is a mere tautology.

The distinction in European thought between bodily and mental pleasures or so-called pleasures of the senses and pleasures of the mind is, as is well known, due to Greek philosophy. It has been respected up to the present day by writers on ethics and is usually connected with the opinion that the latter belong to a better kind than the former. In this short paper we call in question both this opinion and the distinction upon which it is based.

We do not intend to discuss here when this distinction made its first appearance in Greek ethics. It was certainly known to the Cyrenaics, since Aristippus denied the greater worth of pleasures of the mind as compared with those of the body. "They consider bodily pleasures more valuable than those of the mind (*τῶν ψυχικῶν τὰς σωματικὰς ἀμείνονες εἶναι*)", writes Diogenes Laertius about the Cyrenaics, "and bodily pains worse than those of the mind". That is why, he writes later on, they think criminals are punished by the infliction of bodily pain.

Examples by which ethical writers of antiquity, and modern writers who inherited their conceptual framework, used to illustrate the distinction between bodily and mental pleasures and pains were rather poor and stereotyped. According to them, we have to do with a bodily or sensual pleasure in the case of pleasures of the palate or sexual pleasures, whereas pleasures derived from the recitation of poetry, from music or philosophic discussion would be mental. A head-ache

and the sensation of hunger or thirst would represent bodily pains, while we are expected to suffer mental pains in the case of an unrequited love or the anxiety caused by the illness of a near relative.

If someone were to attempt to cover these examples by a general formula, he would probably say that bodily pleasures and pains are derived from physical stimuli and can be sensibly localised in a definite part of our body, as is the case of pleasures of the palate and with pains caused by excessive heat or cold. The situation is different with the pain of humiliation or the pain derived from our confidence betrayed, and with the pleasure of hearing ourselves praised or the pleasure felt when we succeed in fulfilling an engagement in spite of unexpected difficulties. It is obvious that physical stimuli occur in these cases. A compliment we hear is voiced in sounds which affect our hearing; the quality, however, of these physical stimuli is irrelevant. It can as well be expressed in different words of the same language or in a different language and, provided these words are understood, produce the same effect, provoked not by the quality of the sounds but by their meaning.

The proposed general formula, however, encounters difficulties not easy to overcome. One of them is due to aesthetic sensations treated commonly as mental pleasures or pains, although the quoted formula — at least when simple aesthetic sensations are concerned — does not allow us to treat them in a different manner than bodily sensations. In fact, why should the pleasure of having a refreshing drink on a hot day be named a bodily one, while the delight derived from the scent of a meadow in bloom be attributed to the mind? This distinction is the more unwarranted, as smelling has such an important share in the pleasures of the palate. Why should he who takes delight in a harmony of colours be treated as a man having distinguished mental pleasures, while he who enjoys an artfully combined salad be expected to experience only a bodily pleasure?

We shall return once more to this symptomatic inconsistency at the end of our remarks. Meanwhile we should like to dwell a little longer upon the opinion attributing greater worth to the pleasures of the mind. This greater worth can be due to a greater *intensity* or to a different *quality* supposed to give to the pleasure a touch of "nobleness". Few people — we think — would support Aristippus's opinion that bodily pleasures as such are always more intense than those of the mind. Few would also probably support the contrary. On the scale of pleasantness and on the scale of painfulness, bodily

and mental pleasures will succeed one another without any regularity, and comparisons, if at all possible, would give rise to insoluble controversy. There are people who would prefer to have a tooth extracted than to give a bad lecture, while others would choose the contrary.

These are difficulties connected with the opinion that the greater worth of pleasures of the mind is to be attributed to their greater intensity. The position of those who attribute it to a difference of *quality* is still more difficult. Whoever derives pleasure from gossiping, from revenge or from a failure of his enemy, experiences a mental pleasure. As pleasure of the mind it should be more appreciable than the experience of a person who enjoys the taste of a ripe peach.

Here the partisans of the greater worth of pleasures of the mind will protest with indignation that when speaking of pleasures of the mind they never thought of cases like those enumerated above, but that they meant pleasures derived from research work, aesthetic contemplation, pleasures connected with friendship or with a clean conscience. If so, then the opinion about the more honourable position of the pleasures of the mind tacitly involves an important restriction: not all mental pleasures are more respectable, but only those which deserve approval. But then one is bound to think that pleasures *are called* mental only when they are considered to belong to a better sort, which shows that the opinion that pleasures of the mind have a higher position becomes a mere tautology. It is worth noticing that the belief in the superiority of mental pleasures was never connected with the advice of choosing them on every occasion, as nobody would advise a starving man to listen to a Mozart's symphony instead of eating. The pursuit of mental pleasures was recommended only in those cases where some preconditions were fulfilled.

While those who built a hierarchy of pleasures and pains, taking into account their *intensity*, referred generally to examples both of pleasures and pains and especially of physical pains, as they could be easier compared, the partisans of the greater worth of mental experiences in virtue of their *quality* quoted first of all instances of pleasures, as pleasures can be considered as more or less worth pursuing, while nobody, except the masochists, is expected to pursue pains. Since "nobler" means often just "more worthy of pursuit" and no normal man is expected to seek pain, there is not much sense in asking whether the pain of remorse is nobler than the pain of a man deprived of drinkable water following a shipwreck. If one can some-

times sensibly compare the nobleness of two kinds of pain it is usually due to accidental aesthetic associations. Because of these associations the pain of nostalgia can be considered nobler than the pain caused by indigestion.

The distinction of bodily pains or pleasures and that of mental pains or pleasures was meant to bring some order into our ethical thought and has been widely used until recent times. Yet it brings confusion rather than order. No ethical writer would agree that the pain evoked by the success of our rival is nobler than neuralgia. Thus in order to decide which pains or pleasures belong to the better sort it is not sufficient to point to their bodily or mental character, but it is necessary to have a ready hierarchy of values which would enable us to reject as noble the malicious pleasure felt in connection with our rival's failure or the pleasure derived from humiliating other people. Instead of contributing to the elaboration of a hierarchy of values, this widely accepted distinction implies already one which eliminates all disapproved pleasures of the mind from a comparison with bodily pleasures.

There is one, and we think, only one interpretation of the opinion attributing more worth to pleasures of the mind which can give it some sense. According to this interpretation this opinion is not to be understood literally, but simply as *an expression of sympathy with a definite type of man*. It is the sympathy with a man who enjoys thinking and reading, a man who is attracted by the beauty of things and who prefers the pleasure of interesting discussions with friends to the pursuit of gain. John Stuart Mill was thinking of this type of man and of these kinds of pleasures when, in the *Utilitarianism* he opposed "pleasures of the intellect, of the feelings and imagination and of the moral sentiments" to less valuable pleasures "of mere sensation". Having such a definite list of pleasures in view, ethical writers counterposed a man who was pursuing them with a thoughtless being absorbed only by the idea of having a good time. He certainly has also his "pleasures of the mind". But they do not belong to those recommended as worth pursuing, and as such do not deserve being called so.

We have mentioned above an inconsistency in treating simple aesthetic experiences when their "bodily" or "mental" character was concerned. This inconsistency can be easily explained when the distinction between "bodily" and "mental" pleasures is interpreted as a typological one. "Mental" pleasures are pleasures of noble men. A noble man can enjoy the scent of a meadow in blossom, but the pleasure

of excessive drinking is not his. That is why the first experience would be named pleasure of the mind, while the second pleasure of the body, although both are pleasures of "mere sensation" due to physical stimuli and localised in a definite part of our body.

Our remarks concerning the distinction of two differently valued kinds of pleasures were intended to show that this legacy of the past does not deserve to be respected any longer. Whoever insists upon learning which pleasant experiences are mental will, we think, come at last to the conclusion that they are those which are enjoyed by noble men. And since noble men are usually distinguished by their pleasures, a vicious circle seems hardly avoidable.

ABSTRACTS

From "Philosophy of Science"

Vol. 28, No. 1, January 1961

Physics and Ontology, GUSTAV BERGMANN. The recent philosophy of physics is confronted with the new ontology, as it emerges after philosophy proper has fully articulated the linguistic turn. The classical ontologists asserted or denied, controversially, that certain entities "existed." Rather than adding to these controversies, the new ontology uncovers their dialectics. The ontologically problematic entities of physics are of two kinds, represented by forces and particles, respectively. The dialectics has been dominated by eight patterns. Two of these, *independence* and *realism*, belong to philosophy proper. The latter is here considered in order to relieve the philosophy of physics of a burden only philosophy proper can bear. That leaves six patterns: *concreteness* (including the orbit feature), *acquaintance*, *simplicity*, *significance*, *process*, and *model*. The paper sketches how each of these may be used and probably has been used, either explicitly or implicitly, in the recent controversies.

Two Solutions to Galton's Problem, RAOUL NAROLL. Two solutions are offered to the problem of distinguishing "historical" from "functional" associations in cross-cultural surveys. The underlying logic of the mathematical model is discussed and three kinds of association distinguished: hyperdiffusional or purely "historical" association, undiffusional or purely "functional" association, and semidiffusional or mixed "historical-functional" association. Two overland diffusion arcs constitute the test sample; the relationship of social stratification to political complexity constitutes the test problem. A sifting test establishes a bimodal distribution of interval lengths between like types and sifts out repetitions with a lesser interval length than the second mode. A cluster test shows that for the test problem, the "hits" cluster more than the "misses".

A Logic of Questions and Answers, DAVID HARRAH. A logic of questions and answers exists within the logic of statements, if we make the following identifications (roughly): "Whether" questions are identified with true exclusive disjunctions, and "which" questions are identified with true existential quantifications. The question-and-answer process is interpreted as an information-matching game. The question mark is not needed except as a device of abbreviation. Complete and partial answers can be distinguished and various relations of relevance, independence, and resolution defined.

Deciding and Predicting, JEROME RICHFIELD and IRVING M. COPI.

Note on Professor Leonard's Analysis of Interrogatives, etc., J. M. O. WHEATLEY.

A Reply to Professor Wheatley, HENRY S. LEONARD.

Braithwaite's Inductive Justification of Induction, ROBERT C. COBURN.

Mario Bunge on Causality, RICHARD SCHLEGEL.

Vol. 28, No. 2, April 1961

A Panel Discussion of Simplicity of Scientific Theories:

An Introduction to Simplicity, RICHARD S. RUDNER. *The Weight of Simplicity in the Construction and Assaying of Scientific Theories*, MARIO BUNGE. *Safety, Strength, Simplicity*, NELSON GOODMAN. *Inductive Simplicity*, ROBERT ACKERMANN. *On Simplicity in Empirical Hypotheses*, STEPHEN BARKER.

What is Length?, FRANK HENMUELLER and KARL MENDER. One does not only talk about the length in inches of this sheet of paper but also about the length of this sheet, about length in inches and a'out length. A clarification of these and related concepts results from a combination of the theory of the length in a definite unit as a *fluent*, developed by one of the authors, with the other's concept of 2-place fluents. The length ratio L is defined by pairing a number $L(a, \beta)$ to any two objects of a certain kind, a, β (in a definite order). L thus may be described as the class of all pairs (a, β) , $L(a, \beta)$ for any objects, a, β of the said kind. Length of this sheet and length in inches are specializations of this 2-place fluent.

Numerosity, Number, Arithmetization, Measurement and Psychology, THOMAS M. NELSON and S. HOWARD BARTLEY. The paper aims to put certain basic mathematical elements and operations into an empirical perspective, evaluate the empirical status of various analytic operations widely used within psychology and suggest alternatives to procedures criticized as inadequate.

Experimentation shows the "manyness" of items to be a perceptual quality for both young children and animals and that natural operations are performed by naive children analogous to those performed by persons tutored in arithmetic. Number, counting, arithmetic operations therefore can make distinctions that are not inevitably arbitrary, and conceptual operations can obviously have a status as natural events with psychology. If the elements and conceptual operations involved in mathematical systems were not inherent in physiological process, various primitive discriminations could not be possible. Also, since some calculi have a natural status in a given empirical circumstance, the axioms of others cannot be satisfied. Therefore the psychologist when acting as an empirical scientist seeks a calculus having a structure whose elements are isomorphic with natural units of stimulus and response and whose operations are isomorphic with whatever natural processes are involved.

Measurement poses a special problem for the empirical scientist. It concerns but a single class of natural qualities and this only in a limited way. The concept of quantity has a natural counterpart but quantity and measurement are not wholly analogous. Measurement is defined, as H. S. Leonard suggests, as a theoretical activity. Measurement theory has a formal structure but empirical end. Measurement hypothesizes about the position of a particular quality within a definite range of qualities. It therefore is beholden to definite empirical restrictions.

Some hypotheses-making systems use terms and relations per se as the context and

starting point for dealing with discriminable events. Such procedures are "transcendent." In empirical science, questions are part of problem-solving activity and their reference is naturally restricted. In providing description and explanation, psychological researchers frequently use calculi in a transcendent way. This results in theories that are only quasi-empirical and "half" true.

The roles measurement plays in psychology are discussed. Of particular concern are those cases in which the results of measuring or a theory of measurement are used to define the "real" units, or the "real" relations involved in problematic psychological events, and thence to describe and explain behaviors of interest. Metaphysical or ontological usages of measurement sometimes occur. The implication of these arguments with regard to a view of empirical science is discussed.

A Note on Prediction and Deduction, JOHN CANFIELD and KEITH LEHRER. This paper argues against the deductive reconstruction of scientific prediction, that is, against the view that in prediction the predicted event follows *deductively* from the laws and initial conditions that are the basis of the prediction. The major argument of the paper is intended to show that the deductive reconstruction is an inaccurate reconstruction of actual scientific procedure. Our reason for maintaining that it is inaccurate is that if the deductive reconstruction were an accurate reconstruction, then scientific prediction would be impossible.

Putnam's Review of Gödel's Proof, ERNEST NAGEL and JAMES R. NEWMAN.

On the Vindication of Induction, HERBERT FEIGL.

PREDICTABILITY IN LIFE AND IN SCIENCE

by

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It is a significant coincidence that social science tends to assume a universal human need for predictability, and also uses predictive power as the basic criterion of scientific truth. It is claimed here that man's need for predictability often is crossed by a need for uncertainty and chance. Thus it seems doubtful that the methodological canon of predictability can be anchored in the universal usefulness of social predictions. Some important cases of decision-making seem to be more concerned with the past than with the future. The task of the social sciences cannot be completely separated from philosophical problems, since it is part of a continuous endeavour to clarify the image of man.

Social science is part of life. As an activity it cannot be divorced from 'private life' in the same sense as certain other types of 'work' have been isolated from and subordinated to life as merely a way of providing for subsistence. True, it is today possible for an individual researcher to perform social science as 'labor' in Hannah Arendt's sense.¹ But the lack of knowledgeable consumers for the social scientist's product would soon dissolve all standards of procedure, and thereby the task and personnel itself, unless we found our goals and basic canons of procedure in a more comprehensive set of values. These values are not clearly distinguishable from the values that govern our everyday life and actions as citizens.

This is not, at least not always, the case in the natural sciences, including the biological sciences dealing with man. Non-scientists have great capacities for judging the excellence of material products behind which lie enormously complex scientific developments, such as automobiles, TV sets, jet planes and atom bombs. Likewise, most patients show great acumen in judging the outcome of a cure for a disease, although the methods by which it was affected may be based upon completely incomprehensible medical theories. One main reason why such capacity for lay judgment is acute and fairly objective is

that the problem of judging consists in evaluating a material object or a distinguishable physical state in relation to some simple need, for speed, power, vision, absence of pain. It is essentially a problem of adjusting oneself to one other object. Intersubjectivity means an unlimited translatability into worlds of private consumption.

As soon as one enters the social realm, the problem of evaluating a product of research becomes infinitely more complex, although it may be left an open question whether the scientific problems of understanding in themselves are more complex than in the natural sciences. If one were to assume that 'curing the ills of society' is a goal of social science, it is hard to see how laymen could determine that the cure was a success by any other, simpler or more universally available means, than those at the disposal of the social scientist himself. This makes, paradoxically, the social scientist a lonely scientist — in the humanistic tradition — one who cannot, and should not, hope to be vindicated by the obvious, demonstrated mass-utility of his products. He must carry the burden, and enjoy the pleasures, of trying to know what he is to do and why he is doing it, and not trust in external criteria such as the physicists could have done. It is important to remember, however, that the pioneers did not actually rely upon criteria of utility, but went to their work full of private notions of the true, the beautiful and the good.

While natural science has fostered prediction and mastery of an evergrowing area of reality, it has at the same time exposed man to new, previously unexpected areas of uncertainty and risk. Today it is quite as reasonable to say that science has created a basis for man's exposure to ever new uncertainties and for travel into the unknown and possibly unknowable, as to say that he has gained control of nature. With the recent growth of astrophysics the world has become considerably less solidly structured than it has sometimes appeared before. I doubt that this is a mere detour on the road to the certain, known and perfectly manipulable world. Scientists have sought the unknown quite as much for the pleasure of the search as for the utility of making it known. It is not possible to refer to the precedents set by natural science and delegate the criteria of good social science to 'society', to the users of science, or to the future. In setting the goals of scientific endeavours and choosing procedures one can hardly avoid considering, or at one's peril disregarding, what is true, beautiful and good.

Predictability is the catchword of modern sociology. The test of the scientific validity of a statement, a hypothesis, a 'model', lies in

its predictive value, the extent to which it permits us to draw inferences about future states of affairs. There are two main reasons for this anchoring of truth in future confirmation. One, and probably the more basic one, comes out in the image which much (although not all) present sociology draws of human nature. In this image man's need for predictability and mastery of the future looms large. The question of which comes first, the scientific conviction that man actually strives towards prediction and control, or the evaluation that man's dignity and well-being is fostered by such prediction and ability to manipulate the future, is less material in this context. The important observation is the striking harmony between sociology's image of man and of the principles governing interpersonal relations on the one hand, and the methodological canon of predictability as the criterion of scientific truth on the other.

I have attempted in a previous paper to show that men do not quite universally strive for predictability or mastery of their environment. In some areas of life, and possibly in certain situations in all realms, men seem systematically to arrange things so as to preclude predictability and leave events to chance. Solution of some of the very most important problems, including those of life and death, have been delegated to some chance mechanism, a controlled randomizer or merely a sloppy procedure calculated to give the unanticipated a break.² The persistence of such procedures, and their clear and uniform logical structure, suggest that they are the answer to a human need, or maybe rather to some universal condition of human existence. The basic significance of chance and the unanticipated lie probably in their capacity to retain or restore man's dialogue with the environment, to revitalize the sharp sense of outer reality which would be seriously threatened if 'all dreams came true'. Then life might indeed become but a dream within a dream, where the boundaries between the ego and the outer world were dissolved.

It is a very dubious assumption that predictability is universally good, so universal that the notion of truth could be derived from it in the pragmatic tradition. Let us first consider an attempt to introduce the criterion of predictability in a field which belongs to the social realm, and where a maximum of consciousness has been devoted to decisions and actions, but where such criteria of truth by and large have been rejected. I refer to law and legal theory. In recent years a school of legal thought, often called 'legal realism', in Scandinavia associated with the Uppsala-philosophy of Axel Hägerström and the legal theories

of Alf Ross, has claimed that the truth of statements in jurisprudence must in one way or another derive from their power to predict what the courts will do or think in future cases. In Alf Ross's version, probably the clearest and most sophisticated, the criterion of predictability is introduced with the explicit and passionately held goal of making legal theory scientific, i.e. a social science. Ross states his own scientific ideal in these words:

It is a principle of modern empirical science that a proposition about reality (in contrast to an analytical, logical-mathematical proposition) must imply that by following a certain mode of procedure, under certain conditions certain direct experiences will result. The proposition, for example, 'this is chalk' implies that if I place the object under a microscope certain structural qualities shall appear; if I rub it on a blackboard a line will show, and so on. This mode of procedure is called the procedure of verification and the sum of verifiable implications is said to constitute the 'real content' of the proposition. If an assertion — for example, the assertion that the world is governed by an invisible demon — does not involve any verifiable implications, it is said to be without logical meaning; as 'metaphysical' it is banned from the realm of science.

The interpretation of the doctrinal study of law presented in this book rests upon the postulate that the principle of verification must apply also to this field of cognition — that the doctrinal study of law must be recognised as an empirical social science. This means that the propositions about valid law must be interpreted as referring not to an unobservable validity or 'binding force' derived from *a priori* principles or postulates but to social facts. It must be made clear in what procedure the doctrinal propositions can be verified; or what their verifiable implications are.

Our interpretation, based on the preceding analysis, is that the real content of doctrinal propositions refers to the actions of the courts under certain conditions. The real content, for example, of the proposition that section 62 of the Uniform Negotiable Instruments Act is valid Illinois law is the assertion that under certain conditions the courts of this state will act in accordance with the tenor of this section.

These reflections must lead to the conclusion that statements concerning valid law at the present time must be understood as referring to hypothetical future decisions under certain conditions: if an action should be brought on which the particular rule of law has bearing, and if in the meantime there has been no alteration in the state of the law (that is to say, in the circumstances which condition our assertion that the rule is valid law), the rule will be applied by the courts.³

Ross proposes to carry out this programme in law by interpreting every statement on the content of valid law as an hypothesis about the most probable ideological motivation of judges in future cases. Now, a surprising property of Ross's theory, and of other related theories, is that they yield little or no evidence of *curiosity*, of interest in finding out about deeper aspects of judicial thinking or behaviour, which are not already known. It seems that the legal realists and Ross have introduced the canon of predictability as a technical and formal device to save legal theory from non-scientificness and logical meaninglessness, not because they consider new knowledge of judicial behaviour important enough to warrant their own unabashed curiosity. It is significant that these theorists often tend to feel alien to sociology of law proper.

But why is predictability considered a test of scientific validity? Because time is one dimension of reality which permits the scientist to engage in a wager, to make a bet, and lose or win as a consequence of a structure beyond his own control. It is precisely this which makes the outcome appear as an answer from outer reality. The difference between this wager and the one in chance-betting, is that when a bet is once made and won in science, the structure tends to stand, while victory in a game of hazard leaves the world exactly as it was before.

Empirical truths can only be ascertained by applying a cognitive structure, a product of the mind: a theory, hypothesis, guess, or proposition, to previously unobserved and therefore uncertain facts. Our conviction that such structures are not just 'made up', depends upon a minimum element of surprise in the verification, derived from the possibility of refutation inherent in all truly empirical propositions.⁴ The cognitive structure must not be a closed system, capable of explaining everything, this world and its counterproposal, equally well.

The minimal element of surprise can be arranged for in many ways, in law as in other disciplines of social thought. There is no doubt that 'what the courts do in fact' in the future is one of the technical devices by which a legal scholar checks up on his own, as well as upon other scholars' work, to determine their truth value. But there are other recognized ways in which he can arrange wagers with outer reality and conquer the danger of solipsism. He can perform a logical analysis of a statute and arrive at a construction with certain concrete implications, and then go back to available, but hitherto unknown precedents, to see how they compare. If they fit his construction, he knows

that he isn't merely making it up. True, the prediction of future events has a purity, a guaranteed uncertainty, that is not present in these other forms of checking. But the difference is, technically speaking, only of interest if we suspect ourselves and our colleagues of being magicians who put rabbits in hats. Scientific procedure is not, however, in my opinion, a moral test of truthfulness. Those who fabricate Piltown men may even perform a useful function in the scientific community.

If predictive power in a strict sense is to be given a unique status as a criterion of scientific quality, it must be because of the intrinsic interest which appertains to the future. Both the fate of the 'prediction-theories' in the legal profession itself and the manner in which their proponents have argued their case, leave the impression that future events are not uniquely interesting within the structure of legal thought. There is no doubt that legislation has predictability as one of its main objectives. It seeks to make it possible for citizens to predict each other's actions and those of the governmental agencies, administrative as well as judicial. To some extent legal theories elaborate upon legislation and precedent with the intent and function of fostering predictability. The theories of the legal realists, however, evoke the ideal of *maximum predictability*. And this, it seems collides with other goals of legal thinking.

First of all, a shift of emphasis from using future cases as one among several technical tests, to assuming that legal theories deal explicitly and seriously with what judges probably will do, might destroy the usefulness of legal theory as a basis for decision-making and legal action. To be informed about what he will probably do in the future, does not provide the decision-maker with the arguments and justifications which he needs in order to act with self-assurance. This, in spite of the fact that an important theoretical foundation of legal doctrine lies in colleagues studying colleagues. It may give rise to some serious questioning about the goal of predictability within sociology also, when a discipline which today furnishes one of the main bases for social action, takes such an ambiguous and hesitant attitude towards predictions.

Law provides an excellent example of how the assumed universal concern with the future, in eminently practical situations is turned into a passionate concern for the past. The structure of legal thought is such as to make legal decisions essentially decisions about the past.

They are not only a result of the past, but also, as the procedural

norms indicate, phenomenologically oriented towards the past.⁵ When a settlement of the past is achieved, the system of decision-making appears to have reached its goal, to have attained a harmony which it is hard to break for the future course of events. Or, to put it differently, there are a great many decisions that cannot be 'falsified' by future events. A judge's sentence of five years in prison cannot be falsified by the convict's behaviour in prison,⁶ only by new evidence about the past, on the question of guilt. Or, if the verdict is falsified by future events, it is only in so wide a perspective that we are unable to utilize this insight for any concrete understanding of the mechanisms by which decisions are reached.

The problem as stated affects our approach to the 'structural-functional' mode of analysis. Functional analysis assumes that social items can fruitfully be studied in terms of their relatively invariant relationships to subsequent events.⁷ Without the establishment of such relationships, no functional analysis is possible. This is not to say that functional analysis is unconcerned with antecedents. However, it tends to construct and seek the meaning of antecedents as effects in a previous circle of action and feedback. Or, to put it differently, the more sophisticated functional analyses may be reconstructed as causal analyses where social items, e.g. decisions, are perceived as recurrent states and not as single events.

The term 'sophistication' is used intentionally, namely to express 1) that this kind of analysis is rarely achieved in sociology or related disciplines; and 2) that it does not correspond to a phenomenological reality among those involved in the decision or in any other social items. They act in the *now*, from which they may look forward or backward, but hardly in circles. If we want to keep close to phenomenological realities, or to sound behaviouristic methodology, we are often forced to choose between analysis of events in terms either of antecedents or of subsequent developments. This means that the social observer frequently has to decide whether a decision is a choice about the past or whether it is a choice about the future.

From a certain point of view it must appear nonsensical that anybody should make decisions which don't ultimately concern the future. Positivist philosophers, like von Mises,⁸ Philipp Frank⁹ and others have made brave attempts to show that the historical sciences, disciplines which aim at sound decisions on what has actually happened ('Was geschehen ist'), are 'really' concerned with providing bases for predictions. 'Really' means here: in so far as we are interested in

finding a socially workable definition of truth and verifiability, or in so far as we are interested in understanding the social usefulness of retroactive sciences. Similarly, legal and criminological theorists sometimes claim that the value of the court's establishment of guilt must to a large extent be assessed in terms of the future consequences of true decisions on past events. Justice must, from the point of view of the student of society, be assessed in terms of its social usefulness. This imputes to 'society' an interest in 'social usefulness', which I shall claim that 'society' only shows in certain contexts, not in all.

There is a sense in which we continuously develop in the direction of the past. I refer to the explosive development in our understanding and perception of how cultures, man, animals, the earth, the sun, the stars, the galaxies, the universe, came about and took their present shape. But, it will be said, this influence upon the past is merely one in terms of perception, cognition and fantasy. It does not change what 'really' happened. In what sense of 'really'? In the sense that we can observe no material changes in the past, no new action in the past as a consequence of all this new and expanding knowledge of the past. This suggests that we can make no priority-ranking of functional sociological analysis versus historical or simple causal analysis without taking a stand on the question of whether matter is to be given priority over spirit, action over thought and emotion. I should like to propose that it may be wise for some social scientists at least to suspend judgment on this problem, and play with both possibilities. It might lead to such disturbing consequences as the realization that there is a sense in which social systems move not only towards the future, but also towards the past.

No one has evoked the fundamental significance of a social and recognized past with such eloquence as George Orwell. The horridness which Orwell's '1984' conveys, relates to many different aspects of the Big-Brother society. But the real sense of social vertigo appears when the system of history forgery, the fabrication of a new past, is presented: 'If the Party could thrust its hand into the past and say of this or that event, *it never happened*, that, surely, was more terrifying than mere torture and death',¹⁰ speculates the author's Alter Ego, Winston Smith, an employee of the Ministry of Truth, where the historical record is being continuously brought up to date. 'Who controls the past', ran the Party slogan, 'controls the future: who controls the present controls the past.'¹¹ This is one of the fundamental theorems underlying the power structure of the 1984-society. Winston

Smith makes several reflections on the theorem which are highly illuminating for our understanding of the *social* meaning of the past. 'The past, he reflected, had not merely been altered, it had been actually destroyed. For how could you establish even the most obvious fact when there existed no record outside your own memory?'¹² This explains, by the way, why night time and dreams do not become parts of a socially meaningful and cumulative past. It is not a shared past.¹³ 'All history was a palimpsest, scraped clean and reinscribed exactly as often as was necessary.'¹⁴ Therefore, 'Everything faded away into a shadow-world in which finally, even the date of the year had become uncertain.'¹⁵ Orwell returns to the overwhelming significance of the past at the stage where Winston has decided to approach a high-ranking Party functionary, O'Brien, and they are supposedly planning revolutionary activities: 'He filled the glasses and raised his own glass by the stem. "What shall it be this time?" he said, still with the same faint suggestion of irony. "To the confusion of the Thought Police? To the death of Big Brother? To humanity? To the future?" "To the past", said Winston. "The past is more important", agreed O'Brien gravely.'¹⁶

One fairly obvious reason why a definitive and ascertainable past is so vital emerges if we look upon social intercourse as a kind of exchange.¹⁷ Actions call forth actions in others. Social life is a sequence of performances and sanctions. If we stop to observe the social scene at any particular moment, a 'now', we shall find ourselves unable to understand the meaning of what is going on, unless the antecedents are clarified. What is the social meaning of an angry command from a parent to a child, an unkind act, an outburst of tension, a just response? We cannot know without information about the past. And the point is that neither can the actors unless they have a certain record of the past.

Social events have the characteristic that the meanings of actions — perceived as sanctions — are entirely dependent upon a, by then, historical process. Without some consensus concerning the shape of this past, the actions will not take on shared and communicable meanings. Unless consensus can be reached concerning the past, the purposive actions as they occur in a stream of social interaction, become meaningless and absurd. The absurdity may affect the future effectiveness of the actions. But above all, the absurdity may make it impossible to sustain the social relationships, whatever the purposes of the actors are.

Let us compare social events with moves in a game of chess. To move a black knight from b3 to d4 is understandable as an action to get a knight placed in d4. But if we want to go beyond that in our grasp of the meaning of the move, we must know two things: the constituent rules of the game of chess, and the distribution of chessmen at the time of the move. The latter seems to imply that we must attend to the total present 'situation' in order to understand the move, the situation being the stimulus to which the move is a response. At this point, however, the analogy between social interaction and a game of chess breaks down. It is an artifact of chess that the chessmen stay put in definite positions and constitute a 'present situation'. People who interact don't relate themselves to each other in this static way. Their activities lack meaningful *simultaneous* relationships with each other. When someone is moving a piece, he must move it with an awareness of what has happened before. From this point of view a chess-board becomes a frozen memory of what has happened before, of the by now relevant history of the game.

The point of view adopted here has, apparently, a kinship with Wittgenstein's treatment of a definition. A word cannot get its meaning by any straightforward process of pointing, or the like. It takes the notion of a language-game to suggest the establishment of stable relationships between words and objects.¹⁸ The usefulness and the meaning of names, depend upon the name being part of a language-game which has been learnt by the participants in previous conversations. This is not simply to say that we have learned through a process in the past. We have also learned (in the past) that present signs are to be meaningfully related to perceived antecedents (from the point of view of the present).

When people act without memory, without consensus concerning the past, the situation becomes absurd, in the way one so frequently experiences when watching Gogo's and Didi's extravagant non-communication in Samuel Beckett's play 'Waiting for Godot'. The absurdity of the scenes of this play springs from several roots. But one very explicit one, is the profound uncertainty concerning the past, even the very immediate past.¹⁹

Estragon : We came here yesterday.

Vladimir : Ah, no, there you're mistaken.

E. : What did we do yesterday?

V. : What did we do yesterday?

E. : Yes.

V. : Why ... (Angrily) Nothing is certain when you're about.

And so it goes on until a total impasse is reached :

Vladimir : But you say we were here yesterday.

Estragon : I may be mistaken. (Pause). Let's stop talking for a minute, do you mind?²⁰

And then in another context :

Estragon : I'm unhappy.

Vladimir : Not really! Since when?

E. : I'd forgotten.

V. : Extraordinary the tricks that memory plays.²¹

And again :

Estragon : Was it [a tree] not there yesterday?

Vladimir : Yes of course it was there. Do you not remember? We nearly hanged ourselves from it. But you wouldn't. Do you not remember?

E. : You dreamt it.

V. : Is it possible you've forgotten already?

E. : That's the way I am. Either I forget immediately or I never forget.²²

And then the crowning exchange :

Vladimir : What was I saying, we could go on from there.

Estragon : What were you saying when?

V. : At the very beginning.

E. : The very beginning of what?

V. : This evening ... I was saying ... I was saying ...

E. : I'm not a historian.²³

It is, above all, this lack of 'historicism' which gives the play its peculiar atmosphere of timelessness, a static situation without sense and purpose, where all exchanges end in an impasse to be overcome by vaudeville and acrobatics. Bringing the problem down to the microcosmic social system of the two vagabonds, Beckett has tested in some detail the hypothesis put forth by Orwell, that the lack of a past is worse than anything else. Between Didi and Gogo we can see how especially the latter brings his friend to the brink of insanity

by always being willing to 'sell out' and escape from his position by admitting that he may, after all, have been completely wrong about what really happened.

The problem inherent in the 'selling out' on the past may turn out to relate to the basic social structure of law as a triadic arrangement.²⁴ If the past is to be determined with authority, it takes a third agent apart from the two contesting parties. Ideally the third person is an objective outsider with independent resources of power and authority, a judge. But studies of primitive societies have shown that the legitimate involvement of any intermediary may to a certain extent serve the same purpose.²⁵ When the social system searching for a settlement expands from two to three, the new possibility of the alliance of 2 against 1 emerges.²⁶ And the two may decide as they please, unbound by the need for compromise.

As long as there is bargaining going on between two, there will be a strong tendency in the interests of both, to yield and admit that the truth lies somewhere in the middle. In order to achieve the maximum of future rewards, it is usually wise to leave the 'guilty' opponent's self-image relatively unhurt, and receive the compensation not as expiation of past guilt, but as a contribution made in goodwill towards the establishment of amicable relations in the future.

Let me illustrate the two theoretical avenues towards reaching a settlement in a dyadic situation with a conflict of interests. Two people argue about the price of a commodity, say a horse. The seller has presented the horse in a certain way. Now, the potential buyer may concentrate exclusively on the truth of this presentation in stubborn defence of his initial low bid. And the seller may with equal stubbornness claim that the horse has the described qualities, and that the other party's low bid is an insult. Under such conditions transactions are unlikely to take place. Contracts emerge when the parties are willing to forget some of the other party's claims, and instead emphasize their own needs and economic possibilities, e.g. by emphasizing the evaluation of the transaction relative to the future. When both stand to gain in the future by the transaction, a settlement will be reached by the two, without intermediaries, and without insisting on complete clarification of the past, all in the spirit of the 'minimax-principle' of von Neumann.²⁷ But if something happens during the existence of the dyadic relationship, which makes it impossible for both parties to profit by the transaction, a new situation may occur, although it need not. If the commodity perishes, the parties

may go on bargaining about the distribution of the loss in the same way as they did before. But often this proves difficult. For various reasons one or both parties decide that they need an intermediary, a complete clarification of what has happened, with suspension of the minimax-principle. The situation has become legal. The reasons for this transformation may be diverse; and we cannot go into them here. The important point is, however, that at the same time as the past becomes explicitly important, the triadic arrangement is formed. One has entered from the sphere of expediency and utility to that of justice. If there were no such situations where a decision on the past is pressed forward, it is hard to see how a society could sustain stable norms about the relationships between actions and sanctions. The brief illustration above may also suggest why justice has become much more closely associated with the negative sanctions than with social rewards like money and rank.

Legal decisions are essentially decisions about the past. But there may be other reasons why jurisprudence shuns the maximum predictability of legal decisions themselves. Two will be mentioned here. If legislation and jurisprudence put a maximum degree of emphasis upon predictability, it would involve some cost with regard to the concrete justice and equity in the individual cases. Experience shows that it is impossible to foresee all those aspects of future conflicts that may turn out to have some moral significance. Therefore, any attempt to determine in advance with perfect certainty the outcome of litigation is bound to violate, more or less, the retroactive perceptions of the justice of the decisions. The whole of law can actually be viewed as a temporization between the demands for predictability and the retroactive sense of justice. Some legal systems have gone very far in emphasizing the first consideration, like the Code Napoléon, others have gone very far in the other direction like the Barotse of Northern Rhodesia.²⁸ But the point is that no legal system has put exclusive emphasis upon predictability.

The other reason why a rigid criterion of predictability has not won much favour within legal theory, is connected with the intermingling of normative and descriptive elements in legal thinking. If all statements on valid law were to be interpreted as predictions of judicial behaviour, the legal scholar would be bound by an adherence to the *status quo*, which would be objectionable, both from his own point of view and from that of wider social interests. It would deprive legal thinking of a creative, dynamic element which is probably socially beneficial.

This consideration is bound up with the peculiar terminology of law, fusing statements on factual power with enunciation of values and ideals. But some of the same elements enter into the more general sociological language. What is likely to happen is frequently described as the consequence of a normative order. No doubt, the sociologist claims neutrality towards this normative social order, which the legal scholar usually does not claim towards the legal order. The difference, however, is actually only one of degree. Therefore, the sociologist whose generalizations are to be interpreted ideally as predictions, may similarly narrow the opportunities for future action. It would be a mistake, I believe, to assume that the moral justifications for neutral descriptions of the past, the present and the future, are exactly the same. The future gives to the most ardent seeker of truth a liberty which he does not have in relation to the past and the present. If he wants to treat the future as if it had already happened, it is his own choice, to be justified by arguments which do not obviously derive from the basic scientific canons of truth.

I have above adduced some support, at least of a suggestive kind, for two propositions: 1) Man does not unambiguously, maybe not even predominantly, strive for certainty and control. When he does, these strivings are not always oriented towards the future. In part derived from 1) is 2): There are no extra-scientific reasons why sociology should exclusively strive to achieve control and regularity through predictability of the future. On strictly scientific grounds it seems proper to use the criterion of predictability whenever it is possible without seriously misrepresenting that structure of social reality, which seems, on non-methodological grounds, to be the true one. What these propositions and the arguments in their favour do is merely to clear the ground for a new consideration of what the goals of sociology could reasonably be.

The freedom which these points of view leave is in some respects frightening. Sociologists and other social scientists are by now so accustomed to *Wertfreiheit* and the clear distinction from philosophical speculation. And the present line of thought seems to plunge us back into the morass from which we believed that Durkheim and Weber had saved sociology long ago.

This is, however, where we belong. The aim of sociology is to contribute, from its specific angle, to the eternal efforts to clarify, enrich, and question the image of man. Each of the behavioural sciences tends to construct its own image (or images) of man: economic

man, political man, psychological man, and of course biological man. I do not consider it justified to try to supplant these images by a new 'sociological man'. But it is obvious that sociology has made a contribution, and in the future may contribute more, towards an understanding of human nature and of the social realm. And it would be an advantage if these contributions were made with open eyes, with an awareness of the perspective within which the concrete findings have their greatest significance. This perspective can hardly be quite unphilosophical. It is philosophical in at least two respects. Firstly in the sense that it requires of the social scientist a scope of awareness transcending what is often assumed to be the proper concern of a scientific specialist. Secondly, in the sense that he cannot take for granted any simple, technical test of the truth of his theorizing, e.g. its 'predictive power'. They may have very little such power and still contribute a great deal to understanding. And they may have a good deal of predictive power and still be insignificant. It means that the sociologist must be a creative methodologist, not merely a specialist applying a recipe.

One main reason why sociologists can often predict the consequences of certain states of affairs, is that these states of affairs have been purposely brought about in order to produce the consequences. Members of society have through their own planning and their own subsequent observations, verifications and falsifications, built up a cognitive structure, bearing some semblance to a scientific theory. In so far as the social scientist can build further upon these common-sense theories, society's theory about itself, he has a key to important regularities, of which predictability may well be taken as the focus. But I have argued above that social man behaves only in some, albeit important areas in this purposive way. Any attempt, therefore, to stretch the predictability criterion beyond these areas — their limits are largely unknown — may result in a misrepresentation of the nature of human behaviour. This danger is greatly increased by the origin of most social scientists in cultures which heavily stress a utilitarian outlook; and by their belongingness even to the subcultures within these, which are the main bearers of this ethos. A sociology produced by fishermen from Northern Norway or by Andalusian peasants might have been fundamentally different. The leading social scientists are people with tenure and right of pension.

If legal decisions had been made consistently with the aim of controlling the future, it would have been much easier for a sociologist

to predict the consequences of legal decisions. The judges would then have done so much of the work for him. Since they haven't, however, it may be doubted if the right approach in a sociological analysis would be to describe legal decisions with this goal in mind. A clearer understanding of why judges lack responsibility for the future consequences of their decisions, seems to me more important than more knowledge of the possible causal bonds between present decisions and future states of affairs, although I shall not claim that the latter problem is without interest.

If people in some situations attend to the past more than to the future, it is the task of sociology to delimit the situations and the circumstances appertaining to such an orientation. If there are irregularities in social life, it is a task of science to describe them, although this may present terminological problems. But there is so much evidence of unsystematic elements in social life, that to ignore them, or treat them as mere deviations from the essential social order, would be a serious misrepresentation. Men believe in one thing and its opposite at the same time. They do one thing today, another tomorrow. Often they do their very best to achieve the unexpected, or to erect barriers, through secrecy or conflict, so that social units do not take on the character of systems in any strict sense. All these things have to be recorded, regardless of whether they contribute to make society more predictable or not.

NOTES

¹ Hannah Arendt, *The Human Condition*, Doubleday Anchor Books, New York 1955, pp. 71 ff.

² 'Chance in Social Affairs', *Inquiry*, Vol. 2, 1959, pp. 1-24. Cf. also Thomas Mathiesen, 'The Unanticipated Event and Astonishment', *Inquiry*, Vol. 3, 1960, pp. 1-17. The phenomena of chance and uncertainty have been analyzed from a somewhat different angle in John Cohen, *Chance, Skill and Luck*. Penguin Books 1960.

³ *On Law and Justice*, University of California Press, Berkeley, Los Angeles 1959, pp. 39-41.

⁴ Cf. Karl R. Popper, *The Logic of Scientific Discovery*, London 1959, pp. 78-92, and from a more psychological angle, Michael Polanyi, *Personal Knowledge*, Chicago 1958, e.g. pp. 120 ff., 177, 309 ff.

⁵ Vilhelm Aubert, 'Legal Justice and Mental Health', *Psychiatry*, Vol. 21, 1958, pp. 101-13.

- ⁶ Cf. Vilhelm Aubert and Sheldon S. Messinger, 'The Criminal and the Sick', *Inquiry*, Vol. 1, 1958, pp. 137-60.
- ⁷ Cf. Robert K. Merton, 'Manifest and Latent Functions', in *Social Theory and Social Structure*, Glencoe, Illinois 1949.
- ⁸ Richard von Mises, *Kleines Lehrbuch des Positivismus*, Haag 1939.
- ⁹ *Das Kausalgesetz und seine Grenzen*, Wien 1932.
- ¹⁰ *Nineteen Eighty-Four*, A Signet Book, New York 1950, p. 39.
- ¹¹ p. 29.
- ¹² p. 30.
- ¹³ Cf. Vilhelm Aubert and Harrison C. White, 'Sleep: A Sociological Interpretation', *Acta Sociologica*, Vol. 4, 1959, p. 52.
- ¹⁴ '1984', p. 33.
- ¹⁵ p. 34.
- ¹⁶ p. 134.
- ¹⁷ George C. Homans, 'Social Behavior as Exchange', *American Journal of Sociology*, 1958, pp. 597-606.
- ¹⁸ Ludwig Wittgenstein, *Philosophical investigations*, Oxford 1953, p. 5.
- ¹⁹ It is this kind of absurdity which underlies an ingenious, still unpublished, experiment by Harold Garfinkel. Students were given random 'yes' and 'no' answers to questions of an important personal nature. The subjects gave evidence of vigorous denial of absurdity, attributing meanings to the absurd answers by relating them to the antecedents, the questions.
- ²⁰ *Waiting for Godot. A Tragicomedy in Two Acts*, An Evergreen Book, New York 1954, pp. 10-11.
- ²¹ p. 33.
- ²² p. 39.
- ²³ p. 42.
- ²⁴ Vilhelm Aubert, 'Løsning av konflikter i par og trekant' (Conflict Resolution in Dyads and Triads), *Tidsskrift for samfunnsforskning*, Vol. 2, 1961.
- ²⁵ E. Adamson Hoebel, *The Law of Primitive Man*, Cambridge 1954.
- ²⁶ Cf. *The Sociology of Georg Simmel*, Ed. by Kurt H. Wolff, Glencoe, Illinois 1950, pp. 135 ff.
- ²⁷ John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior*, Princeton 1944.
- ²⁸ Max Gluckmann, *The Judicial Process among the Barotse of Northern Rhodesia*, Glencoe, Illinois 1955.

THE ETHICS OF RESISTANCE TO TYRANNY

An attempt to formulate some of the dilemmas involved*

by

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Navigare necesse est
Vivere non est necesse
Pompey

On December 20, 1940 the following discussion took place between Mr. *R.*, the Nazi-backed Norwegian Minister of Justice, and Judge *S.*, a member of the legitimate Norwegian Supreme Court:

S.: The proposal you have made to me has two aspects. One of them is of a personal character, and I will get it out of the way at once. For I suppose you do not seriously expect me to join you in the fraud you are planning¹ — to collaborate with you in order to give this transaction a semblance of legality. But let us pass on to the second issue: the duty which the judges of the Supreme Court have as guardians of the Constitution. Now, this Constitution would have been of little interest if it had no popular backing... But today the people have rallied, more loyally than ever before, to defend the fundamental ideas of the Constitution. More or less clearly, every man has grasped the importance of the principles that safeguard human dignity... Therefore, the people stand together — against you. You have but a vanishing minority on your party rolls. And still fewer there would have been if no one had been forced to join you.

R.: Yes, I admit that we have had to use some extraordinary means.

S.: And notice this: All the best people are on our side... The only new members you have attracted are those who used to set their sails to every wind.

* This paper was presented to The Sixteenth Conference on Science, Philosophy and Religion in their Relation to the Democratic Way of Life, held in New York City, September 1, 1960.

R. : I admit that we are rather weak quantitatively, and perhaps even qualitatively. . . . But there has been a kind of idolatry of justice. There are other values, too — just think of the bad shape of our supply system at present.

S. : That may be so, but there is a word saying that man does not live by bread alone. And today we see that this word really means something to the Norwegians.

R. : But in Germany the Reichsgericht is functioning on the basis of the Weimar Constitution, without getting in conflict with the government.

S. : Yes, but one of the most eminent German jurists of this century, Dr. Walter Simons. . . . says in his preface to the German translation of J. Beck's work on the American political system that the Weimar Constitution can survive only if Germany gets a strong and independent Supreme Court, which, like the American Supreme Court, has a right and a duty to control the operations of the assembly and the executive. And there you have in few words the essentials of free government. The experience of centuries has taught us that whenever one party — and still more, one man — has got the whole and uncontrolled power, arbitrariness has been the unavoidable result; corruption has followed. The reverse of arbitrariness is exactly what we call justice. But that man (pointing to a picture of Quisling) has not the faintest idea of what justice is.

— — —

R. : Do you mean that it would be harmful if our people accepted the national socialist ideas?

S. : I think that no greater disaster could happen to a people . . .

R. : But imagine the irreparable loss to our judicial system if our prestigious Supreme Court should have to make room for a qualitatively inferior court.

S. : In the long run, that will not matter much. The important thing is that people's sense of justice is not damaged.

— — —

R. : I understand that I cannot change your mind.

S. : For me, this is religion.²

The resistance against the nazification of Norway was based on legal, utilitarian, democratic, and religious grounds. Questions of evaluation had continuously to be decided, but basic ethical dilemmas were seldom involved. For even if Norway was strongly opposed to any use of violence, it was commonly accepted that killing, as part of a national defense, is not immoral. Basic conflicts like: killing is permitted / killing is forbidden, may have been experienced by some individuals, but on the group-level the problems seem to have been primarily of two kinds: (1) what shall we lose and what shall we gain

by this rather than that policy? (2) how ought this or that group to *present* its position to the Germans? Utilitarian arguments might be used in private discussions, but the nation as such could not present its position in this ridiculous way: 'We are against the nazification of Norway because we believe that it will produce a lesser balance of pleasure over pain than our own democratic way of life.' As a national platform legal arguments were more useful: the nazification is in conflict with the Constitution. In contact with German officials, some groups did not even present their position in this way, since they did not master the legal technicalities involved. They would say 'I cannot act otherwise, it would be against my conscience.' The function of intelligence, when confronted with tyranny, may sometimes be to find formulations which make discussion impossible.

In order to be able to formulate some of the ethical dilemmas involved, we have to withdraw from the historical arena and concentrate on what we may call an ideal representative of Western culture.

An ideal representative

Western countries have, *inter alia*, been influenced by the three following systems of ethics:

1. the Jewish-Christian,
2. the Greek-humanistic, and
3. the utilitarian system.

Let us now postulate a nation, *X*, which is an ideal representative of these traditions. The norms and value-statements are not only accepted by the inhabitants but they are deeply integrated with their personalities. The inhabitants are sensitive to ethical conflicts and consider it very important to try to find out how they ought to act.

The choice-situation and the alternative policies

Let us now suppose that the following situation is given: *X* is threatened by attack (or has already been occupied) by a foreign power which is a tyranny in the sense that it will try (that it tries), even by the use of terror, to force the inhabitants to accept its authoritarian way of life.

X considers the following alternative policies:

1. Military resistance.
2. Submission and acceptance of the authoritarian way of life.
3. Resistance involving sabotage, destruction of material objects and

demonstration of hostility toward the enemy, but not physical violence toward human beings.

4. Reconciliation based on love of the enemy, trying to make him act more altruistically toward the occupied country.
5. Civil resistance aiming at the maintenance of one's own way of life and opposition to the authoritarian values. Love is not demanded and hate is not forbidden.

Group ethics vs. individual ethics

Our problems may now be formulated thus:

(1) What kind of dilemmas,³ if any, will the decision-makers in *X* encounter when they try to apply the three ethical systems indicated on p. 150 in order to decide which of the above alternatives ought to constitute the group-policy?

On the individual level the question is this:

(2) What kind of dilemmas, if any, will an individual encounter when he tries to apply these ethical systems in order to find out which policy he should recommend to the decision-makers, or which policy he should adopt once the group-policy has been decided?

A person maintains, e.g., that alternative 5 is the way of least evil for the group as a whole, but seeing that the group actually chooses 1, he is confronted with a new situation. What is right for him in that situation cannot necessarily be deduced from what is right for the group as a whole. That is one of the reasons why we must distinguish between group ethics and individual ethics.

Three ethical methods

What kind of dilemmas the decision-makers in *X* will encounter will, *inter alia*, depend upon their ethical method. By an *ethical method* I mean the procedure which a chooser, according to a certain ethical system, has to carry out in order to come to a decision regarding what he ought to do in a certain choice-situation. The three ethical systems indicated above imply different methods. We must distinguish three types:

A. *Decisive emphasis on the motive*: the chooser must try to discover the motives from which he will be adopting the alternative courses of actions open to him, and then choose the one springing from the motive which, according to his ethical system, is the right one, or the best one.

- B. Decisive emphasis on the action:* the chooser must study the characteristics of the alternative courses of action — their motives and consequences are irrelevant — and choose that which is obligatory or right according to his ethical system.
- C. Decisive emphasis on the consequences:* the chooser must propose hypotheses about the consequences of the alternative courses of actions which are open to him in order to find out which one is most efficient in relation to the values of his system. An action is right if, and only if, no other course of action is more effective in the implementation of these values.

According to one plausible interpretation of utilitarianism, an action is obligatory if, and only if, it produces a greater balance of pleasure over pain than any other action possible to the agent. Hence, this system puts decisive emphasis on the consequences.

The Jewish-Christian tradition includes all three methods. We ought to act out of love (*A*). We must not kill (*B*), and we ought to relieve suffering (*C*). Greek-humanistic tradition is more difficult to classify, but at least methods *B* and *C* seem to be included.

First-order dilemmas (conflicts within one method)

The question of which norms are included within the Jewish-Christian, the Greek, and the utilitarian systems cannot be given a definite answer, since it depends, among other things, upon our interpretations of the systems and the specificity of the norms. In this paper I shall limit myself to norms which are more or less obvious instances of the above traditions.

Method A

Suppose the decision-makers keep consistently to method *A* when they try to decide on the five alternatives. What kind of dilemmas will they encounter?

'Act from love and benevolence,' 'Do not act from fear,' 'Hatred is better than cowardice.' These are some of the more important norms which may be interpreted in such a way that they emphasize motivational factors. But they are not formulated as applying to group-decisions, and the policy-makers must determine, more or less independently of these norms, whose motivation ought to be decisive. Ought they to choose that course of action which most of the inhabi-

tants will be taking out of love? Or the one which most of the decision-makers will take from this motive? Or should they choose the alternative which the highest number of persons will be doing from the *same* motive, even if this is morally inferior to love? Or should they in some way compare the moral quality of the motive with the proportion of the population which will be motivated in this way?

And which motive ought to be taken into account? Their motive when they vote on the issue or first make up their mind? Or the motive from which they will be acting during most of the conflict? Or should they disregard the question as to which motive they *actually* will have, and rather try to do the right act from the right motive? What is then the criterion of right action?

Suppose they choose military resistance out of love for their own nation and non-violent reconciliation out of love for the enemy, which love ought to be preferred?

What does 'motive' mean? Does 'motive' mean those anticipated consequences for the sake of which one adopts the action — what we might call the *intention*? Then what is the justification for disregarding the rest of the anticipated consequences and for being satisfied with one's anticipations rather than trying to find out what the consequences will be? Or does 'motive' mean the psychological cause (or causes) of the act which may or may not coincide with the intention? Is it not often the case, moreover, that an action has mixed motives in both these senses of 'motive', e.g., depending upon which layer of the personality we choose as our point of reference?

Finally, can the inhabitants, in any important sense of the term 'motive', know in advance which motive they will have? Ought they perhaps to change from one alternative to another if they discover that they were mistaken about their own motives? If so, what is the difference between this method and ordinary craziness?

Method *A* has its roots in the ethical traditions which we consider. But it is useless as a method for decision-making. (I have found no symptoms of its use in the conflict, referred to above, between Norway and Germany.) But this does not imply that the motive is of no moral interest whatsoever. First of all, it may be of significance if we want to judge an action which has been done. And even in choice-situations we must sometimes calculate with the possible motives, since the effects will partly depend upon their nature. Non-violent resistance, e.g., will have different consequences if chosen out of fear of the use of violence, than if chosen because one believes this alternative to be

morally right. Moreover, if one considers an attitude of love valuable, then it is valuable even if it is realized in the chooser himself, and in time before the consequences of the action.

Method B

Both Jewish-Christian and humanistic ethics contain norms or imperatives implying method *B*. Some of the more important ones are: 'You shall not kill', 'Do not resist evil!', 'Fight your enemies!', 'Do to others what you would have them do to yourself!', 'Act on the maxim which you can will as a universal law!', 'We ought not to prefer our own lesser good to the greater good of others', 'Let no innocent suffer!', 'It is better to suffer unjustly than to act unjustly', 'Death is better than slavery'. These norms can be interpreted so that the motives and the consequences are irrelevant when deciding which course of action ought to be adopted. Whether they apply to individuals only or also to groups is a controversial issue, and whatever answer is given, doubts and guilt feelings will result. Suppose the government in *X* decides to use them. Then it seems that they ought to choose war (alt. 1), since they accept the norm 'You ought to fight for your country.' On the other hand, alternatives 2, 3, 4, and 5 have the support of 'You shall not kill.' 'Do not resist evil' seems to argue in favor of submission (alt. 2), whereas 'Death is better than slavery' argues against it. Alternatives 1 and 3 seem to be compatible with 'Fight your enemies!' and incompatible with the appeal to helpfulness, which argues for alternative 4. 'It is better to suffer unjustly than to act unjustly' may be interpreted as forbidding 1 and perhaps 3 and permitting 2, 4, and 5. All five alternatives are compatible with plausible interpretations of Kant's formula, and incompatible with 'Let no innocent suffer.' 'Do to others what you would have them do to yourself' seems to argue in favor of submission, if it is interpreted as including 'if you were in their situation' — except, of course, if you should *want* resistance.

In order to avoid these problems we may say that the norms do not state duties, only *prima facie* obligations. Hence there are no real, only apparent conflicts. We have a *prima facie* obligation not to kill and a *prima facie* obligation to defend our freedom. In order to find out what is our actual duty, we must compare the obligations with each other in order to see which alternative most fully realizes the most important ones. But how can we compare them? What methods do we have for finding out which obligation ought to prevail? Can our

comparison be guided by the ethical norms we are considering? Or must we go outside these rules in order to reach a conclusion? If the norms are formulated so unprecisely that they are more or less commonly accepted — as we have done — then a decision in favor of any of the five alternatives is both supported and opposed by important norms and the final decision cannot be derived from a consistent norm-system. Though Jewish-Christian ethics is basically so opposed to violence, that if we agree to apply it to group-conflicts, it puts the burden of proof on those who are in favor of it.

The above choice-procedure is implied by the conjunction of our three ethical systems. It is, at least in principle, practicable. But it pays no attention to the consequences of the actions and is therefore unacceptable as the only consideration.

During the German occupation Norwegians sometimes *presented* their views in its terms. In the dialogue quoted, Judge *S* said 'For me, this is religion,' which probably implied 'This *is* my position. It is quite useless that you discuss its consequences.' And the teacher's protest against the nazification of the schools was formulated thus: 'I cannot take part in the education of Norwegian youth according to the directions of N.S.U.F., since this would be against my conscience.' In both these cases, the method seems to have been used in order to avoid discussion.

Method C

According to group-egoistic, hedonistic utilitarianism the question is this: Which of the five alternatives will produce the highest balance of pleasure over pain for the inhabitants? According to universalistic hedonism, the consequences for humanity as a whole must be taken into account. Other norms can also be formulated — either in a group-egoistic or a universalistic form. For instance: 'You ought to choose that course of action which will produce a higher balance of ethical perfection than any other action you can do,' 'You ought to choose that course of action which will produce a higher balance of such values as pleasure, knowledge, virtue, wisdom, and love than any other action you can take,' 'You ought to choose that course of action which, more than any other action you can take, preserves and furthers the tolerant and democratic type of personality.'

The power-holders must decide, first of all, which consequences shall be taken into account, and whether they shall choose a universalistic or a nationalistic frame of reference.

Suppose they accept universalism and such values as pleasure, knowledge, virtue, wisdom, and love. Which of the five alternatives will produce the highest amount of these values for humanity as a whole? In order to find out something about this, they must ask such questions as these: How many will be killed or injured if we choose military resistance? And how many if we choose, e.g., civil resistance? How likely is it that nuclear weapons will be used? That other nations will be brought into the conflict? Does our population have the moral resources necessary for continuing its non-violent resistance in spite of terror? To what degree will military resistance necessitate governmental changes favoring an authoritarian regime? And what are the chances that this development can be counteracted after the war? How will the use of civil rather than military resistance affect the chances of long-term reduction of violence and the creation of a world-community? And how will the inhabitants' self-respect be affected? Under what conditions can we use the short-term consequences as reliable symptoms of the long-term consequences?

The final decision depends upon answers to factual questions like these. But the social sciences do not provide us with the theories from which they can be deduced. Even if the choice-situation is described much more concretely than we have done, the answers will partly be based on guesses. Furthermore, even if the results could be exactly calculated, the above value-descriptions are so abstract that it is nearly impossible to determine what the consequences will be in terms of these values. And the problem of comparison is still unsolved. Suppose one alternative gives us freedom but a large number of dead, and that the other alternative leads to occupation for an indefinite period of time but a lesser number of killed, how do we compare these gains and losses with each other? How do we find out which alternative will produce most values on the whole, and what does 'on the whole' mean here?

The above choice-procedure is probably the one most frequently used in situations of the kind we consider; at least, it seems to provide the decision-makers with their language and their ways of argumentation. But since teleological ethics has been unable to give acceptable test-procedures for such statements as 'Alternative *x* produces, on the whole, a higher amount of values than alternative *y*,' it is illusory to believe that method *C* can always reduce the quest for right policy

to two issues: (i) the clarification of values, (ii) the efficacy of certain alternatives in relation to these values. Nevertheless, the difference between methods *B* and *C* remains, for according to *C* we decide on which action is right after having studied the consequences of a number of alternative courses of actions, whereas, according to *B*, we only consult a certain codex.

Teleological argumentation seems to have been the dominant method of discussion within the Norwegian resistance-movement. As an example of the kind of formulations on the basis of which decisions had to be made, take the following one, written in 1943 in connection with a controversy between the leaders of the military and the civil part of the resistance concerning the appropriateness of guerrilla warfare: 'M. (one of the leaders of the secret military organization) is not convinced that the enemy will start shooting hostages throughout the country . . . That they will shoot may be a better argument than that they have done so. The uncertainty, however, is conspicuous.'⁴

Second-order dilemmas (conflicts among the methods)

Most likely methods *B* and *C* will both have some impact on the decision-making. So here is a source of new dilemmas and conflicts. For within teleological ethics any action may be right. To torture millions of innocent persons is right if it is the best means to the values of the system. Within method *B*, on the other hand, there may be absolute prohibitions, e.g., against torture or the killing of innocent persons. No matter how favorable the consequences would be, the actions are forbidden. The decision-makers in *X* will probably be in doubt as to whether they have the right to try to solve their problem in a purely utilitarian way or whether they should accept certain absolute prohibitions. Few will go so far as to maintain that even the highest amount of happiness for mankind cannot justify the suffering of one innocent person, although it seems to me quite clear that Dostoevsky, in raising this problem, is an exponent of the ethical traditions we consider. But many of us will hesitate when we, on a utilitarian basis, are asked to accept that there is no limit to the amount of suffering we ought to inflict on our enemy if so doing produces most values 'on the whole' and 'in the long run.'

The normative problem

What ought to be *X*'s policy according to my own view?

If we knew in advance that military resistance would lead to world war and that world war would lead to the annihilation of the human race, we would have no choice at all. A universe with rebellious slaves is better than one devoid of life. I should also quite definitely favor occupation if the alternative, war, would destroy our own nation, and yet the human race could survive. Moreover, if occupation only leads to temporary slavery, either because the authoritarian state is not devilish enough, or because the occupied country is able to conduct a successful civil resistance, then, again, the advantages of the non-violent alternative are obvious.

But actual conflict-situations will not necessarily be of these kinds. We cannot be certain that nuclear weapons will be used. And we cannot be certain that our civil resistance will be successful — even as a means of preserving our own values. Perhaps we shall end up in a concentration camp eating drugs which break down our resistance completely and make us scream that slavery is freedom. These empirical uncertainties contribute to the difficulty of decision-making.

Let us assume that our military resistance would lead to victory and a loss of human lives comparable to that suffered by England or the Soviet in the past war. And let us assume that the authoritarian state would try to break our resistance through terror and concentration camps. Here we do not have a choice between violence and survival, since we survive even if we go to war. But we have a choice between killing and watching others being killed.

If our only goal is the long-term reduction of violence, we ought to be the first to use it if it would reduce the amount of violence. For in this case we have based our reluctance to use it on an empirical premise, and we are faced with a problem of calculation: how many will be killed if we take to war? and how many if we do not? If we can save 1,001 children from being killed by killing 1,000 ourselves, we have, on this view, a duty to do so. That we *may* have to use violence, however, is not to say that we must do so. For we may argue that violence produces violence, even if it seems to prevent it in the short run. It strengthens the tradition of violence, reduces our inhibitions and guilt-feelings so that we more easily use it another time, and it humiliates the enemy and makes him want to humiliate us in the same way.

Nevertheless, on this view it may, *theoretically*, be our duty to kill

an indefinite number of persons. There seem to be two possible ways out of this situation. We can maintain that large-scale violence never will lead to long-term reduction of violence. We base our position on an empirical premise, but we claim that it is always true. This seems to me unscientific and irresponsible. The other possibility is to say that we have no right to interfere with life and death in the above way. We have no right to kill 1,000 children in order that 1,001 may live. It is better to suffer unjustly than to act unjustly. This, it seems to me, is the way we must take: there is a limit regarding what we ought to do to others even for the sake of our own or other people's survival. Do not most of us accept this rule under some conditions? If we could save our lives only by torturing to death millions of people, death would be better than a life bought at such a price. But is not modern warfare different from this situation only in degree?

The important thing, therefore, is to strengthen our civil ability to resist authoritarian regimes. This is not only a question of training persons for the experiences of an occupation, but also a question of organizing our institutions in such a way that the loyalty to democratic values is widespread and founded on stable and enduring motivations. Judge S., in his discussion with the Nazi-backed Minister of Justice, was firm and courageous partly because he knew which values he accepted, and because his values and knowledge were deeply rooted in his personality. Such attitudes are not built overnight. To strengthen them should be our common interest. For even if our country goes to war, we may be defeated and occupied. Then it is crucial that submission is not considered as the only possibility.

Let me add, however, that I have made the case difficult for myself. We cannot exclude the possibility that nuclear weapons will be used. And neither Germany nor the Soviet, nor any other modern dictatorship, has succeeded in killing people's drive to freedom.

Individual ethics

Let us suppose that *X*, in spite of all this, chooses military resistance. What shall they do who recommended civil resistance? They do not necessarily have to be conscientious objectors. If their recommendation of group-policy was based on an absolute prohibition against killing or the acceptance of a limit as to what we are allowed to do toward others, they should refuse to fight no matter what the group does, and no matter whether their decision increases or decreases the amount

of violence. But if it was based on universalistic utilitarianism, the question is this: which of the following two alternatives will produce the higher amount of values: (i) everyone takes part in the military defense, even those who recommended civil resistance, (ii) only those take part in the defense who recommended military resistance as a group-policy? Which alternative ought to be preferred will depend upon the circumstances. A difficult situation emerges if the number of pacifists is too low to decide the group-policy but high enough to influence the chances of military victory. In that case, the minority decides the fate of the nation.

Conclusion

For the sake of discussion I present my concluding remarks as seven assertions.

1. Philosophers have paid insufficient attention to the problems raised by the application of ethical systems to groups and the behavior of individuals as representatives of groups. Especially have the ethical problems pertaining to group-conflicts been ignored.
2. We sometimes must choose and decide. Hence we cannot avoid using a choice-procedure. We can only choose what kind of choice-procedure we shall have. The statement that the ethical systems of Judaism and Christianity, humanism, and utilitarianism are irrelevant to the behavior of groups and nations is in itself a normative statement, and, *a priori*, not more defensible than the contradictory one.
3. Ethical decisions must be made on the basis of an empirical analysis of the situation and in view of one's norms and value-statements. We must not require that the system permits rigid subsumptions. It helps to make us aware of aspects worth considering. That is all — but what could be better?
4. The development toward worldwide identifications is furthered by those nations which even today act on the basis of norms which take humanity rather than one's own nation as the frame of reference.
5. The ethical systems under consideration lead to serious dilemmas with regard to the choice of policy of resistance to tyranny. The most important dilemma is this: Shall we accept, as utilitarians, that any action may be obligatory if it only produces more value than anything else we could do? Or, should we supplement utilitari-

anism, as I think we must, with, e.g., the statement that it is better to suffer unjustly than to act unjustly?

6. Accepting this norm, civil resistance seems to be preferable to military resistance.
7. Civil resistance presupposes widespread loyalty to democratic values and must be based on stable and enduring motivations.

NOTES

¹ R., together with Terboven — the German Reichskommissar for Norway — were planning to have the Supreme Court packed with Nazi judges. They demanded the dismissal of the older members, while some of the younger ones should be retained in order to give the operation an air of legitimacy. The actual conflict was due to the Court's persistence, in spite of the German occupation, in passing judgements on the constitutionality of new laws and on their accordance with international law.

■ F. Schjelderup, *Fra Norges Kamp for Retten*, Oslo, 1945, pp. 223 ff.

■ The term 'dilemma' is taken in a wide sense, referring to a difficult choice in general, not only to a choice between two alternative difficulties.

■ F. Schjelderup, *Over bakkekammen*, Oslo, 1949, p. 128.

THE INQUIRING MIND

Notes on the relation between philosophy and science*

by

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There is nothing, either in the recent developments of philosophy or in the development of the sciences, which should prevent philosophy from continuing its role of mother-science and the sciences from influencing methods and conclusions of philosophers. The inquiring mind respects no boundaries between disciplines except those which are imposed by differences in questions raised. But basic questions, whether raised by philosophers or by scientists, tend to have components requiring co-ordination of research or analysis of highly different disciplines. Both Anglo-Saxon and continental developments in philosophy justify, however, a distinction between cultivating philosophy and being engaged in solving or resolving a philosophical problem, the former comprising the latter.

I

The focus of these notes is the relation between philosophy and science in general and empirical research in particular.

Although attempts to define philosophy raise perplexingly difficult questions, it is worth while at intervals to force oneself to undertake the attempt in order to become more conscious about what one is doing, and, in Professor Austin's phrase, 'why one doesn't do something better instead'. Most occupations derive their charm from harmonic development within a framework one does not worry about. Philosophical activity, however, seems to demand examination of itself, examination of this new activity, and so forth. The unexamined life is not worthy of man, says Plato. One might be tempted to add: and the unexamined examination is not worthy of a philosopher.

In professional analytic philosophy, as in any dominating trend of thought or activity, it is all too easy to be caught by the leading phrases and concepts, the 'fashions' of the school with which one sympathizes,

* This article has been prepared in close co-operation with my colleague Eivind Storheim.

or to prefer the learned or the clever to the wise. This is particularly easy today, when philosophy seems to be technical and institution-alized in universities. The unifying, personal outlook will easily be lacking, or rather held apart as a private concern.

To the charge of 'existentialists' that the analytical philosopher does not tell about what concerns him '*innerlich*' and 'infinitely' as an individual, the accused may retort that according to Kierkegaard nothing is more ridiculous than to try to communicate this in plain, simple prose. An indirect or artistic method is required. The philosopher with an analytical bias wishes to communicate in plain prose, and accepts the consequences. What concerns him '*innerlich*' he will express by art or by other appropriate means, not by philosophical writings.

Every attempt to define philosophy is itself more or less of a philosophical enterprise. What is a definition? There is no consensus about what it means to attempt to *define* something. Thus, the attempt to define leads us far into controversial issues. A vast field of problems traditionally belonging to philosophy crop up immediately. It seems impossible to locate one's own position within a larger framework, since a description of the framework is itself a function of that position.

If we succeed in agreeing about a set of questions that they are philosophical, and that a kind of activity dealing with the questions is philosophical, the further question 'what characterizes the questions and the activity?' seems to belong to the same set. And so does the question whether the last sentence I wrote is true or false. We get a Chinese box system, a hierarchy of meta-questions arising immediately upon posing a first, seemingly innocent question. Meta-philosophy is philosophy, and so is this very statement.

II

In this and the next section I shall dwell upon the close relation between general speculation and the emergence of sciences in the Greek world and later. For those who are convinced of the intimacy of this relation my words are superfluous, and they are invited to proceed to section IV.

The Ionians, Thales, Anaximander and others, were, according to our textbooks, the first to start rational inquiries about life and the universe. Departing from mythological and theological thought they tried to give rational explanations of the universe in terms of natural

phenomena — in some sense or other of that term. They intended to build systems of knowledge, universal in scope. The systems presupposed empirical investigations which they were neither eager nor equipped to carry out. As a result, the relation between their sweeping hypotheses and their meagre observations was rather loose.¹

It was the Pythagoreans who started to answer more definite problems and tried to work out detailed answers to subordinate questions and to give the answers in the form of more exact theories. They became the founders of musicology and geometry as sciences, and are justly considered the first scientists, at least in the west. Later followed other sciences, such as astronomy, history, and medicine.

What characterized the purely scientific activities in opposition to the bolder speculations of the philosophers was that the thinking of the former had been methodologically disciplined. Taking as a point of departure general and vague hypotheses about the world or man, certain limited problems were selected. The solution of these demanded patient work of observation and calculation as well as detailed elaboration of a conceptual framework narrowly adapted to the sources and avenues of observation. Schematically, what characterizes a scientific problem is that it has a relatively high degree of inter-personal preciseness in relation both to a set of observational sources and a conceptual framework, and that there exist, at least in outline, recognized, formulated methods for its solution. And this is characteristic of a scientific problem in contrast to a philosophical problem.

III

Philosophy in early Greek thought in a sense fulfilled the function of mother-science. Without the unprejudiced research spirit and the conceptual and methodological inventions of the Ionians and Pythagoreans, the systematic treatment of well-delimited problems within mathematics, astronomy and other fields would have been unthinkable. The systematic character of sciences was due to speculation. One should not, however, underestimate the role played by the more unsystematic experience of artisans. Thus speculation and experience went together.

But the role of general speculation as mother-science or science-generator was not exhausted in antiquity. From the Renaissance until today one can see how problems originating in speculative writings, in part of a general and sweeping character, are taken up for closer

investigations by particular sciences, and how new sciences have developed. Also, fundamental changes have taken place within the sciences, in part motivated by philosophical considerations. Let us look at some particular problems which have been treated within special disciplines.

Problems connected with motion and change had occupied Greek philosophers since the Pre-Socratics. Galileo was not satisfied with the traditional varieties of questions asked. Speculation and experiment led him to discover laws to describe phenomena which were not covered by older theories. As he writes in *Two New Sciences*: 'My purpose is to set forth a very new science dealing with a very ancient subject. There is in nature perhaps nothing older than motion, concerning which the books written by philosophers are neither few nor small; nevertheless I have discovered by experiment some properties of it which are worth knowing and which have not hitherto been either observed or demonstrated'.² By his researches he did not answer many of those questions which already had been asked about motion, but he founded modern mechanics. Galileo's interest in motion and change originated to some extent in his philosophical reflections, and the empirical results obtained are relevant to the problems discussed by philosophers. He exemplifies the old role of philosopher-scientists.

In the beginning of this century the results of Einstein were both strongly influenced by and highly relevant to general problems of space and time, and many of the professional philosophers who have later expressed their views on these topics would have profited by an acquaintance with Einstein's ideas. But many professional philosophers continued to discuss problems of space and time without paying attention to the explications these concepts have received in physics. One concept of infinity, to take another example, was explicated by Bolzano and later by Cantor, thus freeing at least one kind of problem of infinity from the frustrating philosophical discussion. These examples remind us of striking, highly ingenious creations of new scientific disciplines or new foundations of old sciences. It would indeed be presumptuous for anybody to view himself as a philosopher trying to develop new scientific fields, if these examples were the only ones. But they are not. On a smaller scale, as pilot studies and small scale projects, philosophic training and philosophic ideas are utilized to broaden and deepen the areas of scientific research. Campbell Crockett is right in warning against taking 'local and exceptional cases as paradigms of philosophical inquiry', referring to philosophy as 'a

mother science which gives birth to new sciences, impatiently and systematically shifting its subject matter and procedures.’³ It is not argued in the present article that philosophy as a whole is, or ought to be, the mother science, but that to be the mother science is one central task. And the single instance of motherhood Campbell Crockett mentions, Leibniz as ‘the inventor of a new science in the field of mathematics’, is not a typical case. One might, from recent works on intentionality, the ego, and social norms, by psychologists and social psychologists, list a large number of small contributions by people trained in both philosophy and social science. Fields reserved until now for philosophical speculation, contemplation and analysis are opened up for scientific research.

New sciences develop mostly out of old ones without the assistance of philosophic activity on the part of the scientists. But throughout history scientific disciplines also have emerged from philosophical thinking. On the other hand philosophical thinking has been furthered by the developments within the sciences.

At the present time there are in both Continental and Anglo-American philosophy strong currents hostile or sceptic towards the prospect of continued interaction in the future. They seem to me to be due partly to an identification of philosophical activity with a small part of it, partly to a general anti-intellectual bias against plain discursive thinking.

IV

Much has been said lately against the prospect of the continued influence of philosophy in the creation of new scientific disciplines.

Is it not the case that the various sciences have divided the world of problems between them? And does not innovation in methods come from the sciences themselves? There are many who answer in the affirmative. F. D. Ramsay gave a characteristic expression of the problematic situation, as conceived by those who are impressed by the advance of sciences. In 1925 he was asked to speak at a Cambridge society for the promotion of philosophical discussion, and answered the invitation in the following manner:⁴

I do not wish to maintain that there never has been anything to discuss, but only that there is no longer; that we have settled everything by realizing that there is nothing to know except science. . . . It might be thought that apart from this technical

philosophy, whose center is logic, there was a sort of popular philosophy, which dealt with such subjects as the relation of man to nature, and the meaning of morality. But any attempt to treat such topics seriously reduces them to questions either of science or of technical philosophy, or results more immediately in perceiving them to be nonsensical.

Take as an example Russell's recent lecture on 'What I believe'.

He divided it (philosophy) into two parts, the philosophy of nature and the philosophy of value. His philosophy of nature consisted mainly of the conclusions of modern physics, physiology, and astronomy, with a slight admixture of his own theory of material objects as a particular kind of logical construction. Its content could therefore only be discussed by someone with an adequate knowledge of relativity, atomic theory, physiology and mathematical logic. . . .

Although these assertions by Ramsay in a sense are true, I do not think they are relevant as counter-arguments. The contention that the sciences have divided the investigation of the world between them is true only in the sense that today, as in the time of Aristotle, there exist *classifications* (typologies) of questions such that any question whatsoever belongs to at least one science. The belongingness is the abstract one of being covered by an exhaustive classificatory system, and has *prima facie* nothing to do with competence of scientists to deal with the questions.

It can be argued in agreement with Wittgenstein that philosophy 'leaves everything as it is.' But the dictum should not be taken as implying a resignation on the part of philosophers from the job of *leaving nothing as it is* on the borderlines between contemplation and analysis on the one side and scientific research on the other. The inquiring mind cannot be divided into a philosophical part leaving everything, and a scientific, leaving nothing as it is.

Even if the classificatory systems proposed today should prove to be final among human scientists, philosophy might still continue its mother-role by taking up questions for investigation which are neglected or ignored by the scientists within whose field the questions 'belong'.

There is little ground, however, to believe that our present systems of classification are final. Either the classifications keep near common-sense distinctions — living things, dead things, men, animals, spiritual and material things — and in that case nothing is said about the prospect of scientific methods, or the classifications are technical and are

based on dominant techniques and conceptual structures. In the latter case the classificatory systems depend upon theoretical views and models, and these do *not* approach a definite system. The development of physics since Einstein has taught us that tremendous shifts in theoretical views and assumptions are possible — and methodologically desirable — and do not affect the stable flow of observational information being more or less effortlessly added to old information. The accumulation of observations has made measures of constants such as gravity tend towards a definite number, but there has not been, and there is nothing that suggests there will be, a similar convergence in the series of theoretical views and conceptual structures in physical science. The same lack of convergence is noticeable in other sciences. Science accumulates knowledge, but its theories do not approximate to a definite content. Therefore classifications of questions do not approximate to a definite content if they — as they usually do — depend upon theoretical views.

In view of this dependence it is not astonishing to see how classificatory systems of the sciences throughout the times show great variations. Although all of them in a sense are 'total', in that they include everything there is, they are based on different principles of classification.

Bacon classifies what he calls 'learning' according to the 'faculties' by which we investigate the subject matter. These faculties are memory, imagination and reason. To memory corresponds history, to imagination poetry, and to reason philosophy and various sciences.

The French scientist d'Ampère worked out a system according to which the subject matter of science is divided into two broad categories, the material world and the moral world. To the first category he gave the name cosmological sciences, to the second noological. Each of these he again dichotomized until he reached a total of 128 sciences. Needless to say, there is no reason for a philosopher to feel chased out of the world by these 128 brain-children.

It is from the time of Comte, I think, that the most influential conception of the departmentalization of science dates. Comte was able to combine common-sense distinctions with technical ones in a happy way. He reckons with 6 fundamental sciences: mathematics, astronomy, physics, chemistry, biology and sociology. Today it seems odd to classify astronomy as a fundamental science on a par with physics and to leave out psychology. The boundaries are at present anything but clear between physics and chemistry. And psychology has come

to stay. Thus certainly some changes have taken place since the day of Comte. But yet, our Comte-inspired divisions of the sciences and their main methods are taken by many as final. The emergence of psychology as a science has impressed many gifted philosophers and made them uncertain of themselves. Ramsay is a good example. 'Genuine psychology . . . is a science of which we most of us know far too little for it to become us to venture an opinion.'⁵ When this attitude is combined with the view that anything within the *field* of psychology belongs to the psychologists, it cuts off philosophers from a vast number of questions for which their training — combined with training in psychological methods — is eminently fitted.

Strong belief in the finality of the structure of the sciences and in the correspondence between the structure of the world and that of the sciences easily leads one to think that there are no ontological surprises in store for man. It also fosters the belief that research into the unknown should at all costs be accommodated by the methods and conceptual structure prevailing at the moment.

V

Let me, in the light of these reflections, answer tentatively a second question: 'Does not innovation in methods today always originate in the sciences themselves?' The answer will be negative. Scientific conservatism sometimes makes perplexity a fault — phenomena should not be too surprising, they ought to conform to our basic expectations. The tendency to consolidate the boundaries between disciplines and the resulting conservatism and inflexibility has been a formidable obstacle to creative thinking. Take linguistic sciences as an example. No science of language has yet taken shape. Today the various researchers in linguistics, psychology, literature, and philosophy are only inadequately informed about each other's results. The grammarians' conceptual schemes seem inadequate for handling the many distinctions introduced by logicians, and the vocabulary of logic has been shown to be far too rough for the many nuances of a natural language.⁶ But very few have looked for integration through mutual accommodation.

Continually to be occupied with philosophy, with unlimited possibilities and with no frame of reference taken for granted, gives one an attitude which is different from the attitude fostered by the common sense of everyday life or the attitude which is instilled by working

within a special science. The philosopher has no homeland, no garden which is his and only his.⁷ He is therefore ready to follow problems wherever they lead, unconcerned about the allocation of particular fields to particular people.

Philosophical training, especially if combined with scientific, has contributed and will continue to contribute to fundamental questions nominally belonging to the field of particular sciences. In order to make a contribution to any science today it is usually necessary to be versed in that science, but more indirectly, a thinker who is professionally a stranger to a discipline may nevertheless *cause changes* in the conception of its fundamentals. This can be seen today in the continental world, where the phenomenology of Husserl, the ontology of Heidegger and the psychology of Sartre — in spite of their 'anti-scientific' attitude — may have brought about changes in fundamental conceptions of psychiatry. (I do not find substantiated the contention of J. H. van den Berg that phenomenology is a new young science.⁸)

In order to illustrate how philosophical speculation influences the development of a new science, we shall give a brief sketch of the development of psycho-physics. It may make us see that the scientific treatment of a subject-matter as well as the field into which it is classified, is in many ways arbitrary.

VI

It is commonly said that psychology was established as a science in 1860, by Fechner's psychophysics. But the questions which Fechner investigated were of a kind that could be classed in very different ways. It was due to Fechner's metaphysics that his experiments were classed as psychology. The establishment of Fechner's law was to its originator the establishment of an exact relation between the realm of the physical and the psychical, the gateway to a science of psychical events on a par with the existing sciences of physical events. Fechner's law was interpreted quite differently by researchers with other philosophical views (Müller, Wundt, and others).

In 1819 measurements of the kind carried out by Fechner had already been used by astronomers assessing the so-called personal equation.⁹ No one thought at that time of relating these measurements to psychology. By this I want to stress that philosophical thinking may give experimental data new and fruitful interpretations, and also to recall

the largely arbitrary nature of classifications based on those interpretations. A piece of concrete research, an experiment, has no special relation to any particular field of questions or science. The question where it 'belongs' cannot be answered independently of a frame of reference that is superimposed on the experiment.

The techniques of Fechner were taken over by others who had quite other philosophical assumptions, or who were not very philosophically minded at all. It is easy to understand that today many find the investigations carried out in psycho-physics trivial, having far too loose connections with the problems with which psychologists ought to occupy themselves. They become of interest only when they are interpreted in a wider context. (We will not, of course, assert that the only context of interpretation which makes the programme of psychophysics interesting is the one which inspired Fechner). Nevertheless, psychology as a science is still generally considered to have been born in 1860, with Fechner as its father.

Let us then turn to the distinction between psychological questions and psychology as a science. The distinction is of crucial importance when one is informed that this or that belongs to psychology, not philosophy, or vice versa. I shall not here try to define 'psychological question'. The important thing is only to be aware that there are psychological questions which have not been scientifically treated, nor could, by the existing kinds of methods, be scientifically treated today. (Such questions naturally arise when one reads Dostojevski.) The realm of psychological questions, whatever textbook definitions are adopted, is immeasurably broader and richer in dimensions than the field which psychologists at any time are actually investigating by their methods. Philosophical inquiry encourages research in areas which do not today fit into the framework of psychology as an established science. Some of the research of Jean Piaget has this character, to take an example. His general orientation is philosophic in scope, his methodology scientific.

What has been said here about psychology by way of example seems to be more or less true of all sciences. The number of unsolved, even unattacked, questions will always be without end, whatever field is chosen. The ratio of scientifically treated problems to the number of untreated ones will be the same whether we consider psychology, or, say, physics. However, psychology is interesting because of its youthfulness as a science and its still clearly recognized archipelagian character. Islands or clusters of islands of questions are directly attacked by

scientific methodology, but they are surrounded by an ocean of open psychological questions.¹⁰

One way of distinguishing psychology or any other non-formal science from philosophy is to say that in philosophy *relations between concepts or conceptual structures* are analyzed, whereas psychologists investigate correlations or other empirical relations between independent entities, the concepts of which are already given or established. This way of tracing a borderline is tried out by philosophers. Scientists have not, so far as I know, shown any inclination towards accepting it — and for good reasons. Their research work is, in a high degree, work of conceptual clarification and invention. In terms of time and effort, the erection and trying out of conceptual structures and of detailed analyses of interrelations may outdo that of collecting and sifting data. One must also take into consideration that collecting and analysing data, and publishing empirical results, are often motivated by theoretical interests in conceptual structures rather than in what is 'found' empirically.

Recently, a professor of philosophy speculated in his inaugural lecture upon the essential *versus* the accidental features of anger.¹¹ This topic, like many others, seems to be common both to psychologists and philosophers. The style and terminology are different in the different camps, but there is nothing that points to any *consistent* difference in methods, subject matter, or aspect of subject matter. A philosopher may be highly interested in studying the conditions of correct or usual application of a term t_1 as contrasted with a closely related term t_2 , but such studies are also relevant for scientists covering the same phenomena, whether they are linguists, physicist or psychologists.

Analysis of conceptual structures does not mainly consist in semantical clarification. In the last hundred years concepts of 'acid' and the great network of concepts interrelated with them have undergone considerable changes due to empirical advances making old concepts unfruitful. Each change has consequences for a considerable part of chemical conceptualization, and it therefore involves much work of conceptual analysis of all kinds. In biology the work of classification involves conceptual investigation on a very large scale, which has given rise to independent disciplines such as taxonomy, cultivated by people who, like theoretical physicists, need do no empirical work whatsoever.

The inadequacy of the above-mentioned *formulae* of differentiation does not, of course, extend to all distinctions that could be introduced

using those *formulae* as points of departure for precizations. All the major difficulties discussed in this article, however, will arise in trying out more complex and adequate distinctions.

We may say that science sometimes develops as an elaboration and testing of philosophical views. But the philosophical tradition is of course not exhausted by activities eventually leading up to scientific disciplines. To maintain such a view is as sterile as the one according to which the philosopher should never follow a question from speculation to science. As soon as it is realized that scientific research is often an extension of the speculations of certain aspects of philosophy, there is no theoretical reason for a contrast between this side of philosophy and science.

So much for the impact of philosophy on science.

VII

Now, what about the impact of science on philosophy? In part it is due to results obtained in science, in part to transfer of scientific method. Science has made methods available for investigating problems which traditionally were inquired into without tested methods. The philosopher who is interested in problems rather than cultivation of disciplines utilises the methods of science for his inquiries wherever it will contribute to a solution of his problem. He need not stop being a philosopher for that reason. A contemporary Spinoza, in part using symbolic logic and not only more or less intuitive inferences, would still be a philosopher.

The same is true of a philosopher in part using empirical methods. If he needs to know the relation between two attitudes, for instance kinds of conviction, or between two functions of the term 'know', he will list his observations and specify his rules of inference. The solution of a philosophical problem usually involves several different steps and different procedures. A point of departure formulation of the problem might be treated with analytical and other methods, and a set of arguments *pro et contra* might be brought to bear on each of the different interpretations or analyses of the point of departure formulation. Some of these arguments may be arguments *a priori*, some *a posteriori*. The collection of relevant empirical data to confirm some of the *a posteriori* arguments does not solve the original problem, nor does it remove it from philosophy. Research is a complex, many-sided affair.

By treating every problem as potentially a problem of scientific research and not merely one for contemplation and highly intelligent discussion, one is likely to find important distinctions and methods which otherwise would never have been found. The history of science in the last generations actually illustrates the impact of scientific techniques upon philosophical research. There is a continuation of philosophical inquiry by means of technifications and a scientific framework, as we have mentioned earlier. The treatment of a philosophical question does not always need a *new* science; more often already established scientific techniques are made use of. Starting from a vague, but central question within philosophical tradition, logical or empirical methods are applied. This has often led to a narrowing down of the original problem, resulting in a far greater specification of the various sub-problems than would otherwise have been the case. Sometimes it would be more correct to say that the original central question is given up and a new question of far narrower scope is substituted — often without this being expressly stated by the investigator. This might lead to the removal of one or more subproblems from philosophy into one of the sciences, leaving the original problem more specific, and the future discussion more fruitful.

A large part of the sentences in philosophical literature express, for some or all plausible interpretations, empirical assertions.¹² Empirical research methods are applicable to many of them. Of these some have already been attacked with success, whereas others — maybe the greater part — never have been.

As an example of problems raised by a philosopher which may be elucidated by empirical research I shall mention Hume's theory of impressions and ideas. When, in his *Treatise of Human Nature*, Hume discusses what constitutes the difference between an impression and an idea, he states: 'The first circumstance that strikes my eye, is the great resemblance betwixt our impressions and ideas in every other particular, except their degree of force and vivacity.' Experimental psychology has investigated this statement and disconfirmed it. Or, to be more exact, Hume's formulation, if interpreted in various directions, expresses psychological assertions, and one of these has been tested and disconfirmed by known research methods. These and other relevant empirical data have not, however, interested philosophers discussing Humean problems.¹³

It is something of an irony that in contemporary analytical philosophy with its strong empirical tradition (Locke, Berkeley, Hume,

Mill) there is an opposition to empirical research as a genuine part of philosophical research, or *vice versa*. Very few universities encourage training and research of a mixed kind. There is, on the contrary, often some pressure in the direction of an either-or. If a dissertation is one of philosophy, there should be no empirical research carried out in support of its central thesis. If such research is relevant, the thesis is taken to be unphilosophical. If it is one of psychology or any other particular science, philosophical analysis is seldom welcome. (This situation is, of course, not characteristic of all centres of learning.)

In most fields of philosophy questions are discussed in a way which does not primarily aim at solutions of any and all parts of the problem that can be tackled. Avrum Stroll has pointed to the situation in meta-ethics: 'Anyone who reads the literature in ethics is impressed by the amount of disagreement among philosophers over the proper analysis of moral terms in their ordinary usage. The situation resembles the state of nature as described by Hobbes. There is a war of each against all.'¹⁴

In face of persistent disagreement it is natural to look for research methods to supplement the dialogue, and thanks to modern scientific developments there is ample opportunity to apply established techniques, at least as a starting point.

Let us look at one typical assertion in ethics illustrating this. R. M. Hare writes 'To ask whether I ought to do A in these circumstances is to ask whether or not I will that doing A in such circumstances should become a universal law.'¹⁵ The crucial expression here is 'is to ask'. Perhaps Hare intends to assert that everyone who 'in these circumstances' asks himself 'Ought I to do A?' also (1) implicitly or (2) explicitly asks himself 'Ought all people in the same type of situation as I am in to do A?'

Perhaps he asserts that there is a kind of synonymy or likeness of meaning between the two questions. If something like this is what he means, it is possible, after making his hypothesis more precise, to devise tests to decide whether he is right or not, or at least to gather evidence confirming or disconfirming his view. The techniques of linguistics and of social science are available for this kind of research. I am inclined to think that carefully gathered evidence would tend to disconfirm his hypothesis — but that is only a guess. The main thing is to get into touch with problems, to grapple with them as best one can, even if it requires extensive research.

Perhaps Hare means that the first question implies the second. Then

it would be important to specify the sense of 'implies' here, and to concentrate on those interpretations which make hypotheses of the kind 'A implies B' testable by techniques that can be used by all participants in the dispute.

Perhaps Hare means that people who hear a person say 'I ought to do A' infer that he holds that everyone in the same circumstances ought to do A. (This may be a sense of 'implies'.) This is an interesting assertion, but where is the evidence? It is easy to argue against it — or in support of it — as long as some kind of unspecified introspection is considered sufficient to gather evidence. And in fact this is how the meta-ethical discussions largely have been carried on. But another method consists in devising various ways of testing the empirical content of the assertions. Why should one go 'discussing' questions which admit of being intersubjectively tested? Hare's assertion can, plausibly interpreted, be tested. In other cases the empirically testable component of a meta-ethical hypothesis may be less decisive than in the case of Hare. However, in case of protracted controversies those components which admit empirical testing should be tested even if this *alone does not strongly* confirm or disconfirm the thesis.¹⁶

VIII

By neglecting empirical research the problem with which one is concerned easily suffers from lack of definiteness or precision. (The same may be the case of neglecting the conceptual distinctions of modern logic.) Thus, discussion of likeness or closeness of meaning (synonymy) as applied to sentences actually occurring in everyday language or in scientific literature has little problem-solving capacity as long as there is no agreement whatsoever about how to test sentences of the kind 'A is synonymous with B' (and a variety of similar kinds) empirically. Without any connection with ways of testing, the words proposed in analyses (criteria, definitions, explications, conditions of use etc.) do not have a minimum of definiteness (co-operational preciseness¹⁷) making it worth while to enter into a discussion of the proposals.

The discussions concerning synonymy have occupied a prominent place in analytical philosophy during the 1940s and 1950s. It has been considered a central problem to give an 'adequate' definition of the term 'synonymy'. Many suggestions have been made, but none met with general approval. And it was finally considered by some to be impossible to find an adequate definition. Thus Benson Mates said:

I am sorry to have to confess not only that I have no definition to propose, but also that it seems to me doubtful that any adequate definition of 'synonymity' — at least for languages sufficiently complex to make the problem interesting — will ever be found by means of the usual armchair methods of philosophizing. We need empirical research regarding the ordinary language in order to determine which expressions are in fact synonymous, and with the help of these data it may be possible to find an acceptable definition of synonymity.¹⁸

Working out conceptual schemes in intimate connection with empirical tests leads to splitting up 'the' problem of synonymity into a family of sub-problems. Various tests or technifications have been introduced, and words or sentences were thus said to be synonymous according to the various tests.¹⁹ Much the same happened to 'synonymity' as has happened to 'intelligence' in psychology.²⁰ The belief in a definite faculty of intelligence and in the importance of defining the term 'adequately' was gradually undermined by positive research by means of a great variety of tests. It is to be expected that the empirical study of closeness of meaning or use or function or job referring to verbal expressions will also undermine the belief in definitions and general criteria of synonymity, analyticity and related terms.

Before leaving the discussion of synonymity it might be mentioned that the horror among certain admirers of Frege of mixing psychological and logical problems has made it difficult to admit that many questions of consistency and analyticity have an empirical component in addition to the logical. This is due to the dependence of these questions upon questions of synonymity. A dialogue between a logician and an empirical semanticist focusing on the empirical presuppositions in so-called logical analysis is apt to be long and rather complicated — as is well illustrated by J. Meløe.²¹

The connection with empirical research does not only affect the analyses taken separately. It affects a wider conceptual structure. This makes it difficult to answer a straight yes or no to the original questions found in philosophical literature and formulated in terms without any relation to the conceptual framework used in research. As a result there is, unhappily, often a feeling that the 'real' philosophical problem is not attacked by research. Careful analysis is apt to show that this feeling is unwarranted. Propositions about 'perception' and 'reason' within Rommetveit's²² and other social psychologists' conceptual framework — to take an instance — have intricate, but

nevertheless clearly stateable and close relations with central philosophical propositions in terms of 'perception' and 'reason'.

The job of stating these relations presumes a combination of philosophical and social science training that is rarely found. The job may be of such a comprehensive nature that it pays to try out a step-by-step multi-directional precization of a central point of departure formulation. Generally, the stronger the precizations the more likely one is to need empirical research in arriving at valid, testable conclusions. Thus, in his *An Inquiry into the Freedom of Decision* (Oslo 1961), H. Ofstad finds social science or natural science findings relevant to nearly all strong precizations of '(The person) P decided freely in (the situation) S'. Further work along the lines of H. Ofstad would include all main kinds of research activities, including empirical investigations in part by well-known and tested procedures, in part by procedures that would have to be tested by pioneer activities.

But what if any tacit assumptions made in formulating a thesis are made explicit and incorporated in the precizations? This seems inevitably to lead to transintentional precizations and ultimately to some logically and empirically untestable formulations. If fundamentals are somehow incorporated, there is no framework within which any tests can be defined and carried out.

Before closing this section it is advisable to make it clear that I do not mean that the only way to treat philosophical problems 'scientifically' is by connecting them with *empirical* research. This would be as one-sided as it is to maintain as 'obviously true' the propositions (1) that 'observations and experiments are irrelevant to the philosophical argument' and (2) 'that the philosophical argument, consequently, is not an argument about something empirical.'²³ Although empirical research has been focused upon in the preceding pages, I do not (of course) hold that empirical methods are the sole scientifically respectable ones. Analytical tools such as have been developed in mathematics and symbolic logic, or formal conceptual schemes (models, explications etc.) worked out by Rudolf Carnap and others are as important. But it seems to me that within so-called empirical trends of philosophy in our century philosophers have concentrated either on formal methods, or on intuition, in both cases undermining the fundamental position of empiricist methodology. Within the ordinary language movement at Oxford one has, so far, paid little attention to intersubjective methods for testing hypotheses about functions or jobs of language, and to the special tasks of scientific

discourse. Negative conclusions about the possibility of using general or special methods seem mostly to stem from lack of preciseness in stating the problems. In this respect I tend to agree with B. Mates,²⁴ and with A. Wedberg, who says: 'The impression that a philosophical analysis cannot be methodically checked is essentially due to the fact that the analysis is formulated much too unprecisely.'²⁵

The role of empirical research and scientific conceptualization in the understanding of ordinary language, is a complicated topic which requires a separate article. In the foregoing I have, perhaps unwisely, touched on this subject. Let me close the discussion before it gets out of hand, by a small remark in connection with a passage in Cavell's article in this periodical.²⁶

The passage reads as follows:

When Mates says, 'Perhaps it is true that ordinarily I wouldn't say "I know it" unless I felt great confidence in what I was asserting . . .', what he says is not, if you like, *strictly* wrong, but it is wrong — or, what it implies is wrong. It implies that whether I confine the formula 'I know . . .' to statements about which I feel great confidence is *up to me* (*rightly* up to me), so that if I say 'I know . . .' in the absence of confidence, I have not misused language, and in particular I have not stretched the *meaning* of the word 'know'.

If one would try to find out whether Mates is wrong in what Cavell asserts he is wrong, one would have to study a sample of occurrences of 'I know it' and its relations to *feelings* of great confidence. Immediately the usual complications of research on feelings announce themselves: Are we to choose a conceptual framework of an introspective psychology or one of some version of behaviourism? And: What is the status of verbal utterances of confidence ('Certainly, I felt great confidence that . . .', 'I did not feel anything special', etc.) in their relations to confidence?

If introspective conceptions are chosen and it is asked whether great (immediately felt) confidence is present in an individual at the moment or just before saying 'I know it', or 'I know that . . .', a wide variety of answers, many of which are negative, will be our harvest. Some variations are spurious in the sense that they are due to variations in the interpretation of the question one has to ask oneself and others (by questionnaires) in order to get introspective reports on the (hypothetical) great, immediately felt confidence.

Some of the negative answers might support the contention that 'I know that ..' may ordinarily be used in such a smooth way that the questions of evidence and questions of confidence in the available evidence are not contemplated even for a fraction of a second. Mostly, when I say 'I know that ...', I do not *feel* confident in certain introspectionist senses. Whether Cavell would find that I in such cases 'stretch the meaning of the word' depends on the kind of concept of confidence he intends to make use of.

So much about certain interesting complications if introspective conceptions plus questionnaires are used. If behaviouristic conceptions of confidence (and showing confidence) are used, other complications arise — all familiar to those engaged in research along behaviouristic lines.

Cavell concludes 'that the philosopher who proceeds from ordinary language is entitled without special empirical investigation, to assertions ... like, 'We do not say "I know ..." unless we mean that we have great confidence ...' Certainly, he is entitled to such assertions: as point of departure formulations they are more or less indispensable. But if we ask: 'Must we really *mean* that we have great confidence? And do we really need to have or even feel confidence?' we are apt to discard the original formulation and start looking for more adequate ones, based on a variety of relevant distinctions. For those who like empirical research there is no reason to avoid conceptual frameworks and research techniques which obviously or presumably are relevant. For those who are not fond of empirical research there is enough to do without referring to such research. What I find unwarranted is any tendency to make a virtue out of the lack of such reference and the tendency to form definitions of philosophy which reject the relevance of empirical research in philosophic argumentation.

IX

The terms 'philosopher', 'philosophy' and 'philosophical problem' have been used extensively in the foregoing. I shall in what follows propose a definition through which they can be eliminated *from the foregoing sections* without change in intended meaning.

Philosophical activity has two forms, a cognitive activity of clarifying and solving problems, and one of contemplation, with the view of permeating the personality with the themes covered by those problems.

The various philosophical systems from Aristotle to Wittgenstein's *Tractatus* are so many answers to the eternal 'disquietude of the mind', to borrow an expression from Eino Kaila. But an answer to a disquietude is not always equivalent to an answer to a question. The answer may be in the form of contemplation, and of conscious awareness of the sources of the disquietude.

Before trying to formulate a definition I offer this loose characterization which functions as a point of departure: philosophy is man's search for and attempt to give an answer to or to contemplate the most fundamental questions or themes, or to offer a total view or discuss the search for a total view of the world and our existence.

However vague the above formulation is, I think it corresponds roughly to the activities carried out by contemporaries of a variety of convictions who are all called 'philosophers'. The characterization also applies in a certain sense to those philosophers who explicitly reject the existence of specific philosophical questions and answers to questions. They are so to say occupied with it in a negative way, or at a metalevel, by discussing, or dissolving, the problems put forth or the total views offered by others.

Let us consider Moritz Schlick and Gilbert Ryle as examples of philosophers who deny that there is a special domain of philosophic problems. Although they differ on many points of analysis they are both agreed that the task of philosophy wholly consists in making concepts and propositions clear. Philosophy, as they conceive it, is opposed both to the great speculative syntheses and to science.

In his article 'The Turning Point in Philosophy' Schlick says:

The great contemporary turning point is characterized by the fact that we see in philosophy not a system of cognitions, but a system of *acts*; philosophy is that activity through which the meaning of statements is revealed or determined. By means of philosophy statements are explained, by means of science they are verified.²⁷

According to Schlick, then, philosophy is conceptual analysis.

But what concepts shall the philosopher analyse? And on the basis of what set of presuppositions and within what conceptual framework? Does Schlick mean that philosophers should analyse all kinds of concepts employed in ordinary language and science? This is certainly not his view. He writes:

Finally, if within a well-established science the necessity suddenly arises at some point of reflecting anew on the true meaning of the fundamental concepts, and thereby a more profound clarification of their meaning is achieved, this will be felt at once as an eminent philosophical achievement.²⁸

I agree that this will be felt as an eminent philosophical achievement. I do not think that Schlick by 'fundamental' solely means fundamental in the sense of being logically prior to other concepts or assertions. He very likely also thinks that those concepts are of great concern to us. If so, he falls under our characterization.

Gilbert Ryle makes a similar point when he writes:

I conclude, then, that there is, after all, a sense in which we can properly inquire and even say 'what it really means to say so and so'. For we can ask what is the real form of the fact recorded when this is concealed or disguised and not duly exhibited by the expression in question. And we can often succeed in stating this fact in a new form of words which does exhibit what the other failed to exhibit. And I am for the present inclined to believe that this is the sole and whole function of philosophy.²⁹

Is there not implicit in Ryle's suggestion that we see the world wrongly, being misled by language? Is it not his programme to get the categories right so that one gets a truer picture of the world? If so, Gilbert Ryle may be said to imply a status of fundamentality to questions clarified by philosophy. The main object of Ryle and many others is to point out that the traditional questions and answers in philosophical literature are not theirs: their own are at a higher level, a metalevel, they are questions *about* those questions and answers. The traditions furnish the material analyzed.

The characterization of philosophy given above may give an impression of the central place occupied by philosophy in our thinking world. However, it cannot in any way give a useful delimitation of philosophy to distinguish it from religion and science. For this purpose we need a definition, and we shall try to formulate one below.

As mentioned above, I wish to consider two different forms of philosophical activity or rather, if one reckons the activity among the conceptual characteristics, two main concepts of philosophy. Both these activities or concepts take their point of departure in the above characterization of philosophy.

The first activity might be called 'problem-oriented' and is the one

which has been traced in this paper from the Pre-Socratics to our days. It consists in formulating problems or questions, analyzing them in relation to various conceptual frames and linking them with empirical or deductive procedures.

I shall try to condense this conception of philosophy as science-generator or mother-science, in the form of a criterion of what is a philosophical problem at a certain time. By taking the time dimension into account, I do not wish to imply that a problem may not *always* be philosophical, but that this constancy is not essential; and that we shall avoid it. Here is a short formulation of the criterion:

A problem is at time t a problem in philosophy if and only if the following two requirements are satisfied:

1. The problem is such that there is at time t little agreement among presumably competent people as to whether any known procedure, or modification of known procedure, might be adequate in order to reach a relatively high degree of agreement in the answers proposed by the independent researchers.

2. There is at time t little agreement among presumably competent people as to what *is* the problem. That is, there is a discussion going on in which the participants tend to assume they discuss the same problem, but none of the formulations offered at time t are, or would be, agreed upon, to be an adequate expression of it.

In this connection it will be helpful to repeat the characterization of 'scientific problem' given above: Schematically what characterizes a scientific problem is that it has a relatively high degree of interpersonal preciseness in relation to both a set of observational sources and a conceptual framework, and that there exist, at least in outline, formulated kinds of methods for its solution. By these requirements we distinguish a philosophic from a scientific problem. The aim of the philosopher when he handles a philosophical problem is, according to the conception of philosophy as a mother-science, to eliminate it from philosophy, that is, to make it no longer satisfy the requirements. When this has been done, we consider it a scientific problem. A psychological problem might, according to the above definition, also be a philosophic problem, or it might be a scientific problem, if the requirements of interpersonal agreement as to formulation of the problem and existing methods for its solution are satisfied. As mentioned on p. 171 above, the realm of psychological questions is immeasurably broader than the field which psychologists are able to investigate by their methods at any stage of development.

This definition of the philosophical problem is formal in the sense that it does not refer to any subject matter. If considered a complete definition, all problems in the fields of the special sciences concerning which an interpersonal preciseness and a kind of procedure for treating them satisfactorily are missing might be said to belong to philosophy. The definition may on the other hand be supplemented by a reference to material content, for instance by adding: *A problem satisfying these conditions must in addition satisfy the following: it must be related to a fundamental problem concerning the world or our existence — in order to be a philosophical problem.* There being no accepted criteria of what is fundamental the denotation will vary with time and place. The contents of textbooks of philosophy have actually changed in the last two hundred years, but not very much and not always because of change in conception of what is fundamental.

X

I shall now introduce the term 'philosophical themes' as distinct from 'philosophical problems', in order to point to another activity of the philosopher. A philosophical theme is a theme covered by a philosophical question (problem) or group of related philosophical questions. The meaning or significance of life, death, freedom, God, numbers — these are all objects of philosophical questions. The themes are subject to reflection or contemplation — without the intention of answering or clarifying any problem. Just as conceptual distinctions and the devising of empirical methods and techniques may be of varying quality, so may reflections be. Reflections may be more or less deep.

Sometimes the relation between theme and problem may be fairly easy to trace. Thus, 'freedom of will' or 'human freedom' may be the nearest one can come to a verbal delimitation of a certain theme of contemplation. By the process of precization, or more broadly by formulating analyses, of 'freedom of will' or 'human freedom' in various directions one arrives at more or less definite philosophical problems.

It is scarcely right to put contemplation of philosophical themes and discussion or research on philosophical problems up against each other as two incompatible activities. It is often the case that philosophical problems develop from contemplation of philosophical themes. But yet I think there is something irreducible about a philosophical

theme which will not be exhausted by whatever problems are separated from it. Let us take death as an instance. One may contemplate death, and during the contemplation utter sentences about death without thereby intending them to be *answers* to definite questions about death. The contemplative function of language is not a cognitive function, but from the time of Plato the two have been used by philosophers. Contemplation may reveal insight, but does not express it. Death as a theme for contemplation may be the concern of philosophers and poets. *Problems* of death belong to physiology, ethics, economics, psychology and so on — and of course also to the philosopher in his problem-solving capacity. The problems of God, that is, *about* God, belong to the history of religion, theology, psychology and so on. God as an object or theme of contemplation is unproblematic — by definition. It does not involve discursive thinking.

The sentences uttered about a philosophical theme may be written in a dry phraseology of discursive thinking, or in a more or less poetically beautiful language. The communication may be direct or indirect, as in the case of Socrates or Kierkegaard. Because in the latter case the impression on the readers is dependent on non-cognitive factors, it is often argued that these sentences do not belong to philosophy, but to poetry. I disagree with this view, because they differ from other kinds of poetry, and because they have other qualities relating them to the sentences of philosophical themes and problems.

Let us take Kierkegaard and his relation to 'Concluding Unscientific Postscript' as an example. I doubt very much that Kierkegaard in any strict sense can be said to formulate and discuss any philosophical problems in that text. In an appendix he stresses that his relation to the text is only that of a 'third person'. He, and not 'Johannes Climacus' has written the text, but he does not accept as his the opinions attributed to Johannes Climacus. But even if we do take the opinions of Climacus as those of Kierkegaard, it may be unwarranted to attribute to Kierkegaard any formulation of a philosophical problem or a discussion (or solution) of such a problem. A central theme of Climacus is that of the subjective thinker, who in his thinking is aware of his own existence as an individual, and who cannot use a system or, more generally, plain informative language for communicating his thoughts, or rather, his *reflections*, but must rely on indirect communication. Now, if Kierkegaard intends to be a subjective thinker, communicating in an indirect way, the plain talk of Climacus cannot be taken as communication by Kierkegaard as a subjective thinker. If we identify

Kierkegaard's role as subjective thinker with his role as philosopher, the formulations of Climacus, even if accepted by Kierkegaard, are not Kierkegaard's formulation of philosophical problems, and do not represent any discussion or solution of such a problem. Kierkegaard is a philosopher, and he cultivates certain philosophical themes, but he scarcely intends to formulate or solve any philosophical problems in or by his writings.

Recapitulating, I might say that the term 'philosophy' as used in the foregoing sections is intended to cover a complex activity (and the results of this activity), namely the search to formulate, analyze and solve problems not (yet) covered by scientific methods, preferably fundamental problems, *and* to engage in contemplation of the themes either opened up by those problems, or giving rise to them.

If, now, a scientist would contend that he is occupied with and intends to continue to occupy himself with themes and problems satisfying the above requirements, my answer would be that in that case he is doing philosophy according to *my* definition. Actually, some scientists have an attitude in their work which is more like that of a philosopher than many who take philosophy as a purely technical affair.

Those calling themselves philosophers or professional students of philosophy have embarrassing comments to make on the definition: why make scientific knowledge an ultimate end of philosophic handling of problems? Many, maybe most, continental philosophers seem to be convinced that there is exact philosophical knowledge in epistemology, ontology and other branches of a stable domain of problems, and that this knowledge is of another kind than the scientific. I am not going to re-enter the discussion of the relation of established sciences to the results of philosophers. It must suffice to say that if theories, let us say in Husserlian phenomenology, gradually attain the status of interpersonally understandable, describable and testable hypotheses, and the results of testing are satisfying, then the phenomenology is *ipse facto* a piece of science. If *Wesenschau* is a good term for the procedure in testing the hypotheses, this constitutes neither an argument for nor an argument against the use of the term 'scientific'. If what some say, that Husserlian phenomenology does already have this status, is true, then it is part of established science (in my terminology). But here, of course, personal convictions dominate the picture. It is, to me, *not* clear that such a status has been reached.

A broad way of conceiving philosophy would be to let each of the

different aspects of philosophy receive the respect due to it. By reducing all forms of philosophical activity to something one is oneself capable of, philosophy is made too small and too insignificant to command respect from anybody.

NOTES

¹ Karl Popper rightly stresses the importance of the creative, imaginative element in Pre-Socratic philosophy and its independence of observation. I take it, however, that he agrees that observation was taken to be highly relevant to their theories, and that they in fact made changes inspired by new observations. (Cf. K. Popper, 'Back to the Pre-Socratics', *Proc. Arist. Soc.*, Vol. LIX, 1959, p. 3.)

² Galileo Galilei, *Dialogues Concerning Two New Sciences*, Ed. Dover Books, p. 153.

³ Campbell Crockett, 'An Attack upon Revelation in Semantics', *J. of Philos.* vol. 56, 1959, p. 111.

⁴ *Foundation of Mathematics*, pp. 287 f.

⁵ *Foundation of Mathematics*, p. 289.

⁶ Compare Austin's views of philosophy as mother-science in connection with the establishment of a science of language: 'In the history of human inquiry, philosophy has the place of the initial central sun, seminal and tumultuous: from time to time it throws off some portion of itself to take station as a science, a planet, cool and well regulated, progressing steadily towards a distant final state. This happened long ago at the birth of mathematics, and again at the birth of physics: only in the last century we have witnessed the same process once again, slow and at the time almost imperceptible, in the birth of the science of mathematical logic, through the joint labours of philosophers and mathematicians. Is it not possible that the next century may see the birth, through the joint labours of philosophers, grammarians, and numerous other students of language, of a true and comprehensive *science of language*? Then we shall have rid ourselves of one more part of philosophy (there will still be plenty left) in the only way we ever can get rid of philosophy by kicking it upstairs'. 'Is and Cans', *Proc. of the British Acad.*, Vol. 42, pp. 131 f.

I certainly agree with Austin's stress on the development of the sciences from philosophy and also share in the hope that a science of language will be established. However, I think he takes too atomistic a view of the established sciences. The analogy with the sun and the planets is misleading in that the sciences are represented as — once established — quite separated from philosophy, and from one another. Science is an extension of philosophical inquiry. Thus the sciences do not exist in such a splendid isolation as do the planets. Moreover, the sciences do not cool down so quietly as Austin suggests. Volcanic eruptions and earthquakes are common. This, however, may be to strain his analogy too far.

- ⁷ Many ardent cultivators of philosophy have objected vehemently to this characterization. They do so on a basis of settled conviction in the realm of metaphysics and being sure that the philosopher has certain definite problems to deal with — problems all his own — which he solves or tries to solve by exact thinking.
- ⁸ I refer to 'phenomenology' as illustrated by J. H. van den Berg in his *The Phenomenological Approach to Psychiatry, an Introduction to Recent Phenomenological Psychopathology*, Springfield 1955. In his *Preface* the author says that 'Phenomenology is as yet a comparatively young science'. His concept of science may of course be rather different from that used in this article. More careful formulations are found in *Existence. A New Dimension in Psychiatry and Psychology* ed. Rollo May *et al.*, N.Y. 1958.
- ⁹ E. G. Boring, *A History of Experimental Psychology*, p. 135.
- ¹⁰ It is of course relevant here to ask a question upon which, however, we shall not expand: what conditions are to be satisfied in order to say that a problem has been inquired into with scientific methodology? There are all kinds of intermediate stages and levels between unrestrained speculations of the philosopher, the experience of everyday life, the insights of the poet, and the meticulous research of a chemist or historian. It is scarcely possible to say in a few words anything of interest about scientific methodology in general.
- ¹¹ Professor Stuart Hampshire, University of London, 25.10.60.
- ¹² See Arne Naess, *Notes on the Foundation of Psychology as a Science*, 2. ed. Oslo 1960, p. 18, where 148 sentences from Kant's *Kritik der reinen Vernunft* are classified according to various categories, such as 'psychological', 'historical'.
- ¹³ For further examples and discussion of this point see Arne and Siri Naess: 'Psychological Research and Humean Problems', *Philosophy of Science*, Vol. 27, No. 2, 1960. Piaget has taken up for testing a great variety of assertions said to be characteristic of empiricists. He is on the whole critical of those assertions and is of the opinion that he has disconfirmed them through research.
- ¹⁴ Avrum Stroll: *The Emotive Theory of Ethics*, Univ. of Calif. Publications in philosophy, Vol. 28, No. 1, Berkeley 1954, p. 78.
- ¹⁵ R. M. Hare: *The Language of Morals*, Oxford 1952, p. 70.
- ¹⁶ For the general relations between philosophy and the empirical methods of the social sciences as conceived in this article, see Arne Naess: 'Philosophers and Research in the Soft Sciences', *Proceedings of the XIth Congress of Philosophy*, Vol. VI. For the particular relations between ethics and social science see Harald Ofstad, *The Functions of Moral Philosophy*, Oslo 1958, reprinted from *Inquiry*, Vol. 1, No. 1, 1958.
- ¹⁷ Co-operational preciseness and empirical tests in a theme further developed in my *Notes on the Foundation of Psychology as a Science*, 2. ed., pp. 22 ff.
- ¹⁸ Benson Mates, 'Synonymity' Univ. of Calif. Publ. in Phil., Vol. 25, pp. 208 ff. (Reprinted in Linsky: *Readings in Semantics*.)
- ¹⁹ Arne Naess: *Interpretation and Preciseness*, Oslo 1953.
- ²⁰ Arne Naess: 'Synonymity and Empirical Research', *Methodos*, Vol. VIII, Nos. 29—30, pp. 9 ff.
- ²¹ Cf. J. Meløe, 'Dialogue on the Hypothetical Character of Logical Analysis', *Inquiry*, Vol. 1, pp. 72 ff.
- ²² Cp. R. Rommetveit, 'Epistemological Notes on Recent Studies of Social Perception', *Inquiry*, 1958, pp. 213 ff.

- ²³ Justus Hartnack, 'Philosophical Analysis and its Functions' *Theoria* 1960, 2. 225.
- ²⁴ Cf. his 'On the Verification of Statements about Ordinary Language', *Inquiry*, Vol. 1, pp. 161 ff.
- ²⁵ A. Wedberg, 'Philosophical Analysis; Types and Aims', *Theoria*, Vol. 26, p. 235.
- ²⁶ Stanley Cavell, 'Must we mean what we say?', *Inquiry*, Vol. 1 1958, p. 180.
- ²⁷ Moritz Schlick, 'The Turning Point in Philosophy', in *Logical Positivism*, Ed. A. J. Ayer, p. 56.
- ²⁸ Op. cit. p. 58.
- ²⁹ Gilbert Ryle, 'Systematically Misleading Expressions' in Flew (Ed.), *Essays in Logic and Language*, p. 11.

OF WORDS AND USES¹

by

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This paper is devoted to an investigation of one variant of the 'use theory of meaning'. It explores the possibility of characterizing the use of a linguistic unit in terms of non-linguistic facts regularly associated with utterances of the unit in question. It is argued that such regularities are associated with only a small sub-set of English sentences, and then only when these sentences occur in 'standard' contexts. An attempt is then made to characterize the relevant sense of 'standardness' in terms of the role of this concept in a theory of language. In the final section of the paper, some consideration is given to the problem of generalizing the theory to cover sentences which are not regularly associated with recurrent non-linguistic features.

If a child asks what we use a hammer for, we can reply 'to hit things with.' We can then point out that a hammer is better adapted to hitting things like nails than to hitting things like mosquitoes, and we can catalogue the physical properties of hammers upon which this appropriateness depends. If we are inclined to be thorough, we may call attention to the fact that sometimes people put hammers to more exotic uses. Some people *throw* hammers. At this point we may stop with a clear conscience. We have said as much about the use of hammers as any child might reasonably want to know.

What do we do if the child asks what the *word* 'hammer' is used for? Notice that this is a question which calls for a quite different kind of answer. (Indeed, it is a not child's question at all, but rather a philosopher's question.) Whatever use we say the word has, we will feel no temptation to enumerate the physical characteristics of the word which fit it for its function. Nor will we mention other uses to which the word could in principle be put. A thrown hammer is a hammer for all that, but if you change the use of a word sufficiently, it stops being *that* word and becomes a homonym. To say that

'hammer' could have some use very different from the use it *does* have is, to that extent, to make a senseless remark.

We could, of course, *invent* a language in which the question 'What is the word "hammer" used for?'² would be in many respects like the question 'What is a hammer used for?' For example, people who displayed picture-hammers when they were in need of real hammers could make comments about their pictures that are like the comments we make about our hammers. Picture-hammers are used to elicit hammers. They are appropriate to this task because they *look* like hammers. And when you are done with them you may use them for kindling.

Philosophers have sometimes been inclined to confuse *that* language (call it L_1) with English and to suppose that the use of pictures in L_1 is an illuminating analogy for the use of English words. This paper is in part intended to indicate the unfortunate results of such a confusion.

Notice that it is difficult to say *what* in English the picture-hammers of L_1 may reasonably be supposed to correspond to. They clearly do not correspond *exactly* to the word 'hammer' because the English word can appear in any of an indefinite number of sentential contexts, only a very few of which need be concerned with eliciting hammers. The word 'hammer' (or its plural) appears six times in the first paragraph of this paper, for example, and in none of these appearances is a request for a hammer in question. Conversely, the notion of 'appearing in a sentential context' is undefined for the picture-hammers of L_1 . L_1 has no grammar, hence, given only the convention described, there is nothing in L_1 corresponding to chatting about hammers, inquiring about hammers, quoting what someone else has said about hammers, and so on.

Very well, but suppose that we say that the picture-hammers correspond to the English sentence 'Give me a hammer!' In this case, displaying the picture corresponds to the performance of saying 'Give me a hammer!' in a situation where there is someone about who presumably has a hammer to give.

Well, why should we say this? Apparently just because saying 'Give me a hammer!' when there's a hammer about, like displaying a picture-hammer to another 'speaker' of L_1 , is likely to get you a hammer.³ So, we are perhaps inclined to say that in so far as the notion of 'use' is to be introduced by an analogy between L_1 and English, 'hammer' doesn't have a use, but 'Give me a hammer!'

does. 'Hammer' may be up to practically anything, depending upon context,⁴ but "Give me a hammer!" is used for getting at hammers.

Thus far we have dealt rather abruptly with a very difficult question, namely whether the theory of use is a theory about words or a theory about sentences. The relevant considerations seem to be these. If a theory of use is to characterize the distribution of the linguistic units in question by reference to such non-linguistic factors as we have assumed to be relevant to the description of the use of pictures in L_1 , then clearly the concept of use must be primarily applicable to sentences. For though there are apparently no characteristics of the intentions of speakers, or of the responses of hearers, or of the speech-scene itself which may be reliably associated with occurrences of particular *words*, the convention which associates various *sentential forms* with commanding, inquiring, describing, and so forth, guarantees that in standard cases there will be non-verbal correlates which may be reliably associated with at least some kinds of *sentences*. This claim will be subject to much modification further on in this paper. Roughly, however, we may say this: if a theory of use is a theory of the co-variance between linguistic types and typical physical situations, then in so far as these co-variances exist, they obtain primarily between sentences and the world, and only secondarily between words and the world.

We must enter a pair of objections. Notice that the sense in which 'Give me a hammer!' may be said to have a use is still different from the sense in which hammers and picture-hammers may be said to have uses, if only because it makes sense to ask of hammers and picture-hammers, but not of 'Give me a hammer!' why they are used that way. The physical characteristics of hammers and picture-hammers are relevant to their function in a way that the characteristics of 'Give me a hammer!' are not.

Second it must be noticed that though we have perhaps found a use for 'Give me a hammer!' when it is uttered in a certain situation — viz. the situation in which there are hammers and donors about — we have still to find a use for the same sentence when it appears in other situations, be they verbal or physical. For example, from a purely linguistic point of view, the sentence 'Give me a hammer!' must be said to occur in any utterance of the sentence 'John said "give me a hammer" but I said "get your own hammer."' Now, whatever we may say about this appearance of 'Give me a hammer!' it is certain that what it is doing is *not* eliciting a hammer. Clearly

something is going on which has to do with hammers, but, equally clearly, the pristine purity of the hammer-eliciting case has somehow got mislaid.

At this juncture several equally unattractive ways of saving our analysis of 'Give me a hammer!' present themselves. We might, for example, say that while 'Give me a hammer!' standing alone has one use (viz. eliciting hammers) 'Give me a hammer!' in 'John said "give me a hammer" but I said... etc.' has another, unrelated use: namely a use in telling what John said to me and what I said to John. This proposal must, I think, be rejected out of hand because 1) it is too easy, 2) it is counter-intuitive, and 3) it misses the point.

It is too easy because it affords a way of discovering the 'use' of any sentence without also discovering anything revealing *about* the sentence. It is counter-intuitive because it ignores the obvious fact that 'Give me... etc.' and 'John said... etc.' are related in some important way. It misses the point because while 'Give me a hammer!' might really be used to get hammers, the 'John said... I said...' sentence is 'used' only to impart information about what John said and what I said. Now, whatever imparting information may be, it is certainly a different kind of thing from getting hammers, and it was only on the hunch that perhaps most language using might be like hammer eliciting that we started talking about sentences having uses in the first place. It is a lame theory which needs to rely upon 'informing uses of language', and it is surely just cheating to attempt to convince one's reader that 'informing' and getting hammers are somehow both 'uses' on the same footing. Notice, for example, that while almost any sentence can be used to inform someone of something, very few sentences are available to a man who wants a hammer. Hence, in a fairly technical sense of 'information' the amount of information imparted in describing a sentence as 'used for eliciting hammers' is very much greater than the information imparted in describing a sentence as 'used for imparting information'.

Another way that we might try to save our analysis of 'Give me a hammer!' is the following. We might say that the use of a sentence is somehow determined by, or dependent upon, or perhaps equivalent to, the set of conditions in which the sentence may be uttered appropriately. At first blush this seems to work for the 'Give me a hammer?' sort of sentence, and this success may account for the popularity of the proposal. One tells the following sort of story: The 'rule' governing 'Give me a hammer!' is that that sentence is uttered appropriately

when and only when a) there is reason for the speaker to believe that there is a hammer in the offing; b) there is reason for the speaker to believe that there is someone about who has access to the hammer; and c) the speaker wants a hammer.⁵ Now, clearly, if 'Give me a hammer!' *does* have a use, the satisfaction of the conjunction of conditions a—c must have something to do with that use. It seems plausible to say that 'Give me a hammer!' is used to do something (viz. to get a hammer) in situations where conditions a—c are satisfied. If the first two of these conditions were *not* satisfied, the speaker would have to find some other means of getting a hammer. Talking would not then do the trick. If the third condition were not satisfied, then, *ex hypothesi*, the speaker would not be after a hammer at all.

If, however, we turn our attention to the 'John said . . . I said . . .' sentence, difficulties arise. The desired parity of analysis fails to be forthcoming. The problem here is that one cannot state conditions under which the utterance of that sentence is *either* appropriate *or* inappropriate, beyond the very general conditions that there needs to be someone about for the speaker to be saying the sentence *to* and (perhaps) that the speaker must believe the sentence he is speaking.⁶ Declarative sentences are, as a rule, appropriate in many situational contexts,⁷ and, conversely, in many situations any declarative sentence is appropriate. (Think of the variety of things that can be said without linguistic impropriety at a cocktail party.) Briefly, declaratives are not helpfully thought of as occurring either appropriately or inappropriately (these categories normally fail to apply) though, of course, they may be expected or unexpected, or true or false. Remember, we are looking for conditions of appropriateness which have something to do with the use (or even, if you prefer, with the *meaning*) of the sentence, in the way that conditions a—c are concerned with the use, or meaning, of 'Give me a hammer!' But when Mrs. Prothero offered the firemen something to read, though she may have been guilty of good manners in excess, she was *not* guilty of a misuse of language.

It is a point about language which philosophers are inclined to overlook that one can say practically anything of discourse length at practically any time without a breach of linguistic propriety. The inclination to ignore this fact is, I believe, one of the more unfortunate consequences of thinking about language as though it were a process analogous to holding up pictures; that is, of attempting to analyze most sentences on the model of the more obvious uses of 'Give me a

hammer!' The difficulty, of course, is that the degree of reality control upon most utterances is very much slighter than is suggested by a consideration of commands, sentences containing demonstratives, performatives, and so on. This works both ways. We cannot, in general, predict *what* is likely to be said from an investigation of the speech-scene of the predicted utterance. (Try to infer the dialogue of a film from the visual action.) Nor can one, in general, predict the characteristics of a speech-scene from a knowledge of what is said. (If this fails to seem completely obvious, consider, for example, what is involved in adapting a play from the legitimate theatre for radio. It is quite clear that very many things that it is somehow redundant to say when the audience can *see* the action must be carefully inserted when the audience can only hear the dialogue. The point of this exercise is that it is not easy to effect a verbal determination of the characteristics of a speech-scene; *id est* to write a speech which can be delivered appropriately in only one kind of situation. So far are most utterances from labouring under strong situational controls that one must often go to extraordinary lengths if one wishes to indicate verbally what the situation of an utterance is.)

Now, it seems certain that these considerations apply as well to the 'Give me a hammer!' case as to the sentence about what John said. There are surely situations in which one might say 'Give me a hammer!' when any or all of the three conditions previously enumerated fail to be satisfied. Thus, King Richard can demand a horse when he knows that condition b is violated and rather suspects that condition c may be. And certainly one might, for any of a number of reasons, go through the verbal performance of asking for a hammer without in fact wanting one. Whether this sort of linguistic playfulness is to count as 'really' asking for a hammer is a question that I wish to leave open. At any event, performances of this sort (I shall call them special effects performances) seem to militate against our formula that the use of 'Give me a hammer!' is to get a hammer, and that it is therefore appropriately uttered only in the kind of situation that we have described.

Notice that creating special effects is a perfectly legitimate kind of linguistic activity. Making joke, indulging in ironies, expressing wishes, and so forth, are all things that one does with language and could hardly do without it. Hence they are all things that a theory of language may reasonably be expected to illuminate. There is no *a priori* reason for supposing that what really counts as language-using

must be such drab performances as asking for a hammer when one knows that there's a hammer in the wings.

We are, then, presented with a dilemma. For one does feel that there is *some* relation between situations in which one says 'Give me a hammer!' and situations in which a hammer is believed to be present and obtainable, which makes utterances of *that* sentence in *that* situation peculiarly worth studying. Philosophers who have talked about the problem of meaning have consistently singled out that kind of pairing as particularly significant.

The situation seems to be the following: if English were as restricted a system as L_1 , we should feel ourselves entitled to say without much hesitation that the use of 'Give me a hammer!' is to elicit hammers in situations satisfying conditions a—c. English being what it is, however, to say this would be to invite an avalanche of counter-examples in the form of cases where English speakers might quite reasonably be envisioned as saying 'Give me a hammer!' in defiance of any or all of our conditions. Clearly each of these examples would be an instance of speakers using 'Give me a hammer!' to do something other than what we feel inclined to say that 'Give me a hammer!' is supposed to do. If we deny the plausibility of such cases, we gravely underestimate the resources of the language. If we admit that the counter-examples hold, we apparently must abandon our claim to have described the use of 'Give me a hammer!' In either case our position seems uncomfortable.

One thing that we might do is shift our ground. We might claim not to have described *the* use of 'Give me a hammer!' but just to have described *a* use of that sentence. This claim, however, is very weak. We may bolster it by adding that the use that we have described is the *standard* use. But though this sounds very much better, if we stop here, all that we have in fact accomplished is to give a name to our intuition that the use of 'Give me a hammer!' at present under discussion really is the important one, and that the other uses to which that sentence might be put are somehow secondary. That this intuition is in fact accurate seems to me indubitable, but an intuition, however accurate, is not a theory of language. What we need are one or two reasons for taking our intuitions seriously.

It is at this point that a number of very bad reasons suggest themselves. We shall consider two. The first, which I suppose should be known as the retort irate, claims in effect that the intuitions of native speakers are self-justifying. It goes as follows: I know what the standard

use of the sentence is because I know the language. (*Id est* I know how to speak the language.) My intuitions about the language are accurate because it is *my* language, learned at my mother's knee and practised for ever so many years since. Thus, if I say that such and such a use is standard, and that such and such another is *not*, you may very well take my word for it. Who after all, could be in a better position to know?

It is curious that Professor G. Ryle, who in his article 'Ordinary Language'⁸ comes very near to adopting a slightly more sophisticated version of the present *non sequitur*, should elsewhere in his writings have provided us with precisely the correct counter-argument to it. To wit, that 'standard use' is part of our language *qua* philosopher but hardly *qua* native speaker of English. Since 'standard use' is a theoretical term, we are in need of a theory to justify its application. It is of course the case that, as English speakers, we are in possession of a very considerable amount of information about the structure of our language. Unfortunately, however, these data are stored in the form of habits which tell us, borrowing Ryle's terminology, *how* to go about constructing and interpreting English sentences, but fail entirely to tell us *that* English is one way or another, viz. to provide us with a theory of English. One cannot, after all, truth-value a habit; and though one may be very good at *speaking* English, one might quite possibly still be very bad at *describing* English.

In short, the most admirable set of dispositions does not amount to a theory, although a coherent description of the speech-habits of English speakers is precisely the sort of theory that we require. Hence, we must do as linguists do: we regard the native speaker with considerable relish as a source of data about the language, but we politely decline to take his *theories* about the language any more seriously than the arguments he advances in their favour seem to warrant. Roughly (but very importantly) one can have reliable intuitions as to what one would say *but not as to why one would say it*. The philosopher who permits 'intuitions' to stand as justifications of the application of such theoretical terms as 'standard use' does not thereby explain linguistic behaviour; at best he adds to the data to which such an explanation is relevant. The metatheoretic intuitions of the informant are, then, to be treated simply as further data in need of further explanation. This also holds for the case when the philosopher acts as his own informant.

We are, the reader will remember, considering bad reasons in

favour of our intuition that the standard use of 'Give me a hammer!' must be the use above described, and we are doing so because we decline to consider such intuitions as self-justifying. Now, one of the reasons most frequently advanced is the following: the standard use must be as noted because that is the kind of use in terms of which such sentences as 'Give me a hammer!' are taught and learned. I shall throughout refer to this as the pedagogical argument. It depends upon combining bad philosophy with questionable psychology, and therefore deserves our attention as a classic of its kind.

The bad philosophy is introduced as follows: let us grant the premise that, as a matter of fact, one always does and only could learn to properly employ sentences like 'Give me a hammer!' in situations where conditions a—c are satisfied. Now we must ask what bearing this implausible pedagogical principle could possibly have upon the question of what the standard use of 'Give me a hammer!' might be.

I want to maintain that to argue from the way a skill is learned to the way in which it is (or must be) used is to overlook the rather well-known phenomenon of stimulus generalization. For example, it is a belief widely prevalent among young boys in the United States that the only way to learn how to use a baseball bat is to practise long hours swinging at very small rocks (the point being that a baseball will then seem a relatively large target). We are not, I think, in this case inclined to say that the standard use of a baseball bat must lie in hitting stones because that's how children learn to use baseball bats. Instead, what we must say is that here, in any event, there can be no inference from the way a skill is acquired to the way it will be employed *after* it is acquired. It seems characteristic of children that in the process of play (or of imitation) they acquire various skills of which the standard adult use will be grasped later, gradually, and by analogy. Or, to put it another way, the standard performance is implicit in, but not identical with, the learned performance in the way that learning to hit a baseball is implicit in learning to hit a rock; in the way that learning to swim is implicit in learning to paddle while wearing a life-preserver.

But now it seems to me that the onus of proof rests with defenders of the pedagogical argument. I think they must show us some reason to suppose that linguistic skills, unlike skills of other kinds, require to be learned by a rehearsal of *standard* performances. For, notice, if they fail to show this there is no longer any good ground for supposing

that arguments from how sentences are learned to their standard use are not simply *non sequiturs*.

I said that the pedagogical argument compounded bad philosophy with questionable psychology. The philosophic error consists in the mistake of supposing that there is an inference from the premise that a sentence can be learned only in a given employment to the conclusion that *that* employment must be the standard one. The questionable psychology is latent in the original acceptance of the antecedent of that inference.

The notion that talking about hammers is a skill that can be acquired only in 'standard' situations (e.g. situations in which there are hammers about) originates in a theory of learning which we have every reason to consider radically suspect. According to this theory, language learning, or, in any event, *first* language learning (the 'Original Language Game' so-called) consists essentially in the rote memorization of pairings of sounds with presented objects. In the purest case the teacher says 'hammer' and displays a hammer. The child somehow makes the connection and files away this bit of information for later reference. Similar pairings are established for 'give' and 'me' (it is a point worth noticing about the theory that it fails to seem natural for words other than concrete nouns) the meaning of the sentence being then determined by the use of the component words.

I am not inclined to attack this theory directly, although it is fairly easy to show that it is woefully inadequate to account for at least *some* aspects of language learning (e.g. the assimilation of grammar). Suffice it to point out that an alternative theory is possible,⁹ and hence that any argument which depends, as the pedagogical argument appears to do, upon the rote-memory theory being true is, to that extent, not air-tight.

Suppose that learning the use of a word (even the use of a concrete noun) turned out to be more like successful hypothesis formation than like the memorization of a pairing for a nonsense syllable. If this were true, then one could no more predict with certainty the conditions under which a child might grasp the use of a word or sentence than one can predict with certainty the conditions under which a scientist will arrive at a true theory. In the former case, as in the latter, the amount and type of information required would depend on the intelligence and inspiration of the investigator. Hence, if we assume that grasping uses might be a matter of forming and

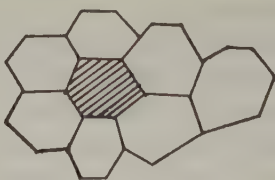
checking hypotheses, there would be no reason whatsoever to grant the initial premise upon which the pedagogical argument depends: namely that words and sentences can be learned only in their standard employments.

Well, but is the hypothesis formation model even conceivably a candidate for a model of language learning? Consider the following cases, upon the plausibility of which I will rest my claim that the rote memorization story *might* be false.

Case I: A father is hanging a picture on the wall of his workshop. His child watches pensively. Opposite him I assume a tool-rack containing, say, a hammer, a chisel, a saw and a screwdriver. I assume also that the hypothetical child does not know the appropriate word for any of these tools. Suppose that after a period of intense but frustrated exertion it becomes evident to the father that he will not be able to get the thumb-tack into the wall by strength of hand alone. So, gesturing towards the tool-rack he says to the child: 'Hammer!'

Now, I want to say that it is not beyond the realm of possibility that the child *might* notice that the father is in need of an instrument heavy enough, hard enough, and broad enough to drive the thumb-tack. And it also seems conceivable that the child might notice that the only available instrument with all these characteristics is in fact the hammer, and might thus, by an admittedly circuitous route, come to the realization that it is hammers that go with 'hammer', and not chisels, saws, or screwdrivers.

Case II: Again the father and child; this time let the father be laying slabs. Suppose that he has completely covered the floor with them except that, having discovered himself to be one slab short he



has left a gap in the centre, resulting in this sort of arrangement: where the shaded area represents a discontinuity in the slabs. Now, I want to say that it seems likely that a child might immediately understand his father were the latter to say 'I need another slab if I'm to get

finished.' even supposing that the child has never heard the word 'slab' before.

Notice two points: first, the obviousness of this case makes the child's achievement appear somewhat trivial. In fact, it represents no mean feat of intellect. The child might have guessed that 'slab' meant the same as 'helper' or 'hour'. Of both of these opinions he would have been disabused only upon the arrival of further evidence.

Second, notice how strongly this sort of case suggests the implausibility of the rote memorization, standard-use-is-teaching-use model. In the present case, the child is supposed to learn a word by attending to the *absence* of the object for which the word stands. Now, if one is committed to the notion that words have uses, I suppose one wants to say that words like 'slab' are associated in their standard use with things like slabs. But the present case, if in fact it represents an empirical possibility, prohibits *both* saying that the standard use of 'slab' is to signal the presence of slabs *and* that standard use is teaching use. For here the word is being learned by observing not a slab but a non-slab; by attending to a place where a slab is not.

I do not, of course, know how languages are really learned, and I am not convinced that at the moment anyone else does either. (Our lack of information on this subject worries psycho-linguists; why does it not worry philosophers?) The important point for present purposes, however, is that if it turns out that the domestic dramas just outlined are at all close to the real thing, then there is no reason at all to believe that words or sentences can be learned only in standard employments. Hence, once again, there seems to be no inference from the way that a word is learned to what its standard use is.

The reader will remember that we are in search of some reasons for thinking that the sentence 'Give me a hammer!' is somehow intimately related to situations in which hammers are in demand. So far, however, all the reasons that we have turned up have been at best suspect. Suppose we begin again.

What we seem to be certain of — though we are not certain *why* we are certain or how much our certainty counts for — are two things. The first is that the various special effects uses of 'Give me a hammer!' are in some important way related to the use that we have been describing as standard (i.e. to the use in which our three conditions are satisfied). The second is that *in* that relation the standard use is

somehow fundamental. What we now need is a theory of the relation in question. This is a kind of problem which linguists very often face, and we may find a way out of our difficulties by considering what linguists have done with theirs.

Consider the following problem. It is characteristic of English that the language frequently permits the same word to play several grammatically different roles. Thus, in English, nouns and verbs are mutually convertible in a surprisingly large number of ways. A word such as 'hunt', for example, may appear as a verb in the sentence 'Hunt the man down' or as a noun root in any of a number of nominalized constructions such as 'The hunt proceeded apace.' 'The shooting of hunters was heard.' 'Hunting is an inhuman sport.' 'The hunting of deer is prohibited.' and so forth. It would be folly to claim that this group of constructions is grammatically unrelated. Indeed, the system of nominalization and verbalization appears to constitute one of the more powerful and striking regularities of English grammar. On the other hand, there is a considerable problem as to how one ought to describe this system. Should one, for example, take the verb form as fundamental and describe the system as a set of rules for nominalizing verbs, or should one take the noun form as fundamental and describe the system as a set of rules for verbalizing nouns? The decision that one makes here will very largely determine the picture of this part of English grammar at which one finally arrives. It is thus in no sense a trivial decision.

What a linguist does with this sort of problem depends to some extent on the kind of theory he is concerned with developing. If he is interested in describing the diachronic evolution of English, then he is committed to taking the underlying form to be that which in fact appears earliest in the development of the language. Suppose, however, that the linguist's interests are synchronic. That is, he is concerned with developing a characterization of the language system as he discovers it to be at present, regardless of the historical development which determined the grammar to its present form. In this case, what kinds of considerations may legitimately govern his choice?

Bloomfield says somewhere that in cases such as the present one what appears in the abstract to be a free option for the linguist will often turn out to be determined by the language system itself. That is, given *one* choice the linguist may find it possible to develop a compendious and elegant description of the grammar of the language, and given the other choice he may not. The linguist then, like any

other working scientist, resolves his difficulty by adopting the theory which is in general simplest, most elegant, most productive, and most consonant with the data available.

Now, I think that this points a way out of our problem about choosing the 'standard use'. In order to make the analogy with the linguist's procedures as strong as possible, let us state our problem in the following way: what we are aiming to do is to discover a characterization of the system of uses to which a sentence like 'Give me a hammer!' may be put which is, as far as possible, simple, revealing, and consonant with the actual practices of English speakers. What I want to suggest is that, if we think of our problem *this* way, then it becomes immediately apparent that such a characterization is most likely to be achieved if we take what we have been calling the standard use of that sentence to be in fact the fundamental one and describe the special effects uses as a system of fairly uniform divergences from the standard use. If, on the other hand, we consider as fundamental a use of 'Give me a hammer!' in which our three conditions are *not* satisfied, it seems unlikely that any revealing characterization of the system of uses of that sentence will be forthcoming. If this assertion turns out to be in general correct, if explanations of use turn out to be asymmetrical in this way, then I think we may claim to have evolved a sufficient justification for our conviction that the standard use of 'Give me a hammer!' is what we have claimed it to be. The significance of this use is discovered to lie, not in the manner in which it was learned, nor in any peculiar relations it might be supposed to have with the non-verbal world, nor even in its putative centrality in the intuitions of speakers, but rather in its special role in descriptions of English. This may, of course, be thought of as a purely conventionalistic solution of our problem if you happen to believe that theories are conventions. I do not.

I shall not, in the context of the present paper, attempt anything so elaborate as a characterization of the various kinds of special effects uses of sentences such as 'Give me a hammer!' in terms of their putatively standard uses. This paper is intended not to solve the problem about standard use but just to show what kind of problem that problem is. At any event, such a characterization would be only half a proof that the standard use of 'Give me a hammer!' is what we have claimed it to be. The other half would consist of a demonstration that the characterization tendered is in some reasonable sense the optimal one. I do, however, want to make two comments about

the special effects uses of sentences like 'Give me a hammer!' and the relation of these uses to the standard use.

The first thing to notice is that the system of special effects uses is in fact bounded. There is a line beyond which the special effects uses of a sentence pass over into oddity,¹⁰ though that line is admittedly shifting and hard to draw. Conversely, when a sentence is used for special effect, other people who speak the language are generally able to tell *which* effect is being invoked. There would be very little point in indulging in ironies, expressing wishes, and so forth, unless this were so. We are not, then, condemned to choose between speech which is pedantically standard and unintelligible soliloquy. But this fact is, it seems, inexplicable except upon the assumption that there exist fairly standard ways of diverging from standard speech. Were this not the case, English really would be rather like L_1 (one symbol, one convention) and our long search for a rationale for distinguishing the standard from the non-standard uses of 'Give me a hammer!' would have been quite unnecessary.

The second point is that, quite aside from one's theoretical commitments, the most natural way of characterizing the sort of speech acts which are standardly divergent (rather than unintelligible or odd) is by calling attention to the ways in which these special effects violate the conventions standardly associated with utterances of their sentence type. To give one example, most of the dictionaries I have checked describe irony by saying, in effect, that it is a conscious and apparent violation of the regularity that the utterance of a declarative sentence implies, in the standard case, that the speaker believes what he is saying. This is, of course, a very gross characterization. Remember, however, that we are at present concerned with utterances of one sentence length where the sentence involved is of a grammatically simple kind like 'Give me a hammer!' or 'Brutus is an honourable man.'

It seems, then, that there may be something to be said for the notion that (some) sentences have uses. What the contention boils down to is apparently that we can state regularities, or conditions of appropriateness, or whatever, which characterize standard occurrences of certain sentences. It is *not* the case that these sentences cannot be used in violation of the associated regularities, but what might very well be true is that the irregular uses of such sentences lend themselves to a relatively simple characterization in terms of the standard ones and not vice versa. So much, then, in defence of

the intuition that the sentence 'Give me a hammer!' is somehow intimately concerned with situations in which a hammer is required.

If we now consider the entire set of sentences which, like 'Give me a hammer!' (and unlike the 'John said...I said...' sentence discussed above) can be paired with conditions of appropriateness, we see that they form a sort of half-way station between a rudimentary sign system such as L_1 on the one hand and English on the other. Unlike the picture exhibiting performances of L_1 , utterances of sentences belonging to this set (to which I shall refer as L_2) may be divergent in characteristic ways. To that extent L_2 manifests something of the richness of ordinary language. On the other hand, the very fact that there *are* conditions of appropriateness upon standard utterances of these sentences is sufficient to distinguish L_2 from English. Utterances of 'Give me a hammer!' or 'Here's a lovely red apple.' are either divergent or odd unless some rather strict conditions happen to be satisfied in the utterance situation (e.g. conditions a—c in the first case and the proximity of an apple in the second). But, as we pointed out above, there are no such situational conditions upon 'John said "give me a hammer" but I said "get your own hammer!"' Nor are there upon 'If I had an apple, I'd eat it in a wink.' The problem of getting from L_1 , a language for which the use theory (understood as a theory of the relation between sentences and situations) is illuminating, to English, a language for which, by and large, it is not, is perhaps the central problem for philosophy of language. I shall conclude this paper with a very brief discussion of one possible solution to this problem.¹¹

The approach in question is an essentially lexicographic one.¹² It starts by noticing that, although English is a very complex system to which indefinitely many sentences belong, it is axiomatic that any arbitrary English sentence can be uniquely specified as a finite ordered sequence of words selected from a finite list (the vocabulary of English). Second, it seems reasonable to suppose that although L_2 is not equivalent to English, a very large number of English words can appear in sentences of L_2 . Hence, the problem of discovering a semantic characterization for English sentences is at least in part reducible to the problem of developing a lexicon for L_2 .

It will be remembered that it is characteristic of L_2 that every sentence belonging to it can be paired with a set of conditions of appropriateness which determine its standard use. The creation of a lexicon for L_2 depends upon that characteristic, for, given any

sentence in L_2 , it seems conceivable that we may determine the meaning of its component words by attending to the way that its associated conditions vary as we substitute for words in the sentence. Thus if, for example, there is a difference between the conditions associated with 'Give me a hammer!' and those associated with 'Give me a file!' it seems reasonable to attribute that difference to a meaning difference between 'hammer' and 'file'. Pursuing this line of thought further, if there is any characteristic of the associated conditions which appears whenever 'hammer' occurs in a standard utterance of a sentence of L_2 and does not appear elsewhere, then it is a plausible move to associate that characteristic with the word 'hammer' in a dictionary description of that word.

Since we are here concerned with what we may call the expansion problem (*id est* with the problem of developing a characterization of English in terms of units defined for L_2), I shall assume that considerations such as those just mentioned might in fact lead to techniques for the construction of a lexicon for L_2 . If this assumption proves correct, the expansion problem is reduced to four sub-problems, which I shall simply list as fruitful fields for further research.

1) How are we to go about expanding the lexicon of L_2 to include words which are in the vocabulary of English but not in the vocabulary of L_2 ? In particular, how shall we handle words which are usually thought of as introduced by explicit verbal stipulation, such as theoretical terms?

2) In precisely what sense are we to think of the lexicon as yielding an interpretation of sentences? In particular, can we define some sense of 'composition' such that a principle of composition will hold for most sentences (i.e. such that the meaning of these sentences will be determined by the dictionary meanings of their component words)?

3) How are we to handle clearly non-compositional sentences (e.g. idioms and poetic discourse)? In particular, can we devise some technique for interpreting non-compositional sentences by relating them to sentences for which a principle of composition holds?¹³

4) Finally, how are we to represent the impact of grammar upon meaning? The lexicographic approach assumes that, at least in the case of compositional sentences, semantic analysis proceeds one word at a time, the meaning of the sentence being in some sense determined by the meanings of the component words. Now, it is fairly certain¹⁴ that this sort of word-by-word analysis is insufficiently powerful for a

characterization of English *grammar*. Hence it appears that an essentially lexicographic approach to semantics may turn out to be out of phase with grammatical theory, thus leading to a most undesirable asymmetry between grammar and semantics. This is, of course, no reason for abandoning the investigation of lexicons. What it *does* suggest is that at some point philosophers may have to abandon their traditional loyalty to *words* as the fundamental linguistic units and begin to think in terms of a semantic theory which might seek to characterize English directly in terms of the sort of simple sentences which constitute L_2 , thus avoiding, or at least localizing, the appeal to dictionary construction. Such an appeal has, of course, been central to the theory outlined in this paper.

NOTES

- ¹ I wish to express my gratitude to Mr. G. Warnock of Magdalen College and Professor H. Putnam of Princeton University for their advice on earlier drafts of this paper.
- ² I adopt the convention of punctuating *within* quotation marks to indicate a complete utterance with a characteristic intonation contour.
- ³ Upon this dreary fact rests the entire burden of the analogy between English and L_1 . Were it not supposed to obtain, there would be no temptation whatever to think of L_1 as a language. That the analogy it licenses is weak at best follows from our inability to indicate *which* hammer-eliciting phrase in English the analogy concerns. We have as much (and as little) inclination to translate the picture display as 'Hammer, please.' 'Quick, a hammer.' 'Would God I had a hammer.' and so on as by 'Give me a hammer!' It is precisely the interesting characteristics of English which do not carry over into L_1 .
- ⁴ Without a specification of context, how shall we tell the noun from the verb?
- ⁵ Conditions analogous to c may reasonably be thought of as associated with the imperative sentence *form*.
- ⁶ Notice that, since both of these are conditions associated with declaratives *in general*, they are not sufficient to characterize the use of the present sentence in particular, whether they are taken singly or in conjunction. This point is important if one accepts the claim that the amount of information imparted by a description of a sentence varies inversely with the number of sentences of which that description is true. There is, at any event, no reason to be satisfied with a description of use which is not sufficient to distinguish the sentence so described from all other sentences excepting synonyms.
- ⁷ *N.B.* This is different from saying (what would surely be false) that arbitrary declarative sentences are appropriate in any *discourse* (e.g. that any declarative can coherently follow any other). The point that I am now making is, in effect, that one can begin any discourse at any time; although having begun a *given* discourse, the sentences that one can utter are restricted by considerations of coherence.

- ⁸ G. Ryle, 'Ordinary Language', *Philosophical Review*, April 1953. In all fairness, Ryle was there talking about use of *words*, not of sentences. I do not, however, believe that this prejudices the present argument.
- ⁹ Cf. N. Chomsky's review of B. F. Skinner's *Verbal Behaviour* in *Language*, 1959, No. 35, pp. 25—58. Chomsky here enumerates in some detail the shortcomings of a conditioning model of language learning and then considers some of the alternatives.
- ¹⁰ Cf. my article "'What Do You Mean?'"', *Journal of Philosophy*, 21 July, 1960, where this point is argued at some length.
- ¹¹ A rather different approach to this problem is outlined in my paper 'Projection and Paraphrase in Semantics', *Analysis*, 21.4, March 1961.
- ¹² The following section is very much influenced by Paul Ziff's admirable book *Semantic Analysis* (Cornell University Press, 1960). However this section is in no sense intended as an exposition of Ziff's position, nor is Ziff to be held responsible for whatever errors or misunderstandings I may be guilty of.
- ¹³ A suggestion which derives from Ziff's unpublished paper 'On Understanding Understanding Utterances'.
- ¹⁴ N. Chomsky, *Syntactic Structures*, Mouton and Co. S'Gravenhage, 1957, contains a number of very persuasive arguments to this effect.

REVIEWS

R. E. Money-Kyrle, *Man's
Picture of his World*. 190 pp. London
1961, Duckworth. 21s.

In one of Nigel Balchin's novels, a character reflects on the oddity of any moderate or modest attitude to the ideas of psychoanalysis: like the notion of God, these ideas, if true, make so enormous a difference that one can hardly escape making them the centre of one's thought and life. It also follows that the inquiry into their validity should be an important concern, and so should their falsehood if they turn out to be false.

In fact, however, the history of thought and indeed our own intellectual climate of opinion are full of ideas and ways of thought which *logically* have this overwhelming power, whose internal claims are such that they ought to force us into some supreme Pascalian wager and commitment about them one way or the other, but which in practice come to be used as small changes of the mind and to live in more or less peaceable coexistence with rival ideas (Marx, Freud, Wittgenstein . . .). At their inception, they may be treated with the seriousness their content calls for, but in the course of time they are somehow watered down and socialised, and learn to live and let live. This argues ill for human consistency and sincerity, but socially speaking it is a great blessing. For logically, Mao is probably right and Krushchev wrong, and coexistence a heresy. But the survival of societies depends on such heresies being accepted. No society and no harmony without doublethink. (It is only through unavowed heresy, for instance, that Muslim states can subscribe to the United Nations charter and refrain from periodic Holy Wars.)

The ideas of Freud have gone through this process of domestication, though happily it is by no means complete or stable. This mellowing is due partly to sheer age, the acquisition of caution-engendering

vested interests on one side and habituation and acceptance on the other, and in part it is deliberately fostered by some practitioners themselves, perhaps in the hope of avoiding criticism. One often hears protestations of modesty, such as that psychoanalysis should not be seen as a world-view, that it is but a technique with limited claims and powers, or that it cannot be expected to aid the psyche in difficulties which have an objective rather than neurotic basis — claims which, whether convenient or not, are I suspect in conflict with the inner logic of the ideas themselves.

The works of Mr. Money-Kyrle are greatly welcome, for these are the reflections of a practising psychoanalyst who is also genuinely aware of the outer world, and attempts to explore the cross-implications with lucidity and candour. Mr. Money-Kyrle does indeed see the world from the vantage-point of psychoanalysis, and a very strange world it turns out to be; and it is interesting to see the world from this perspective, its objects acquiring quite different proportions and positions from those they habitually possess. But at any rate it *is* our common world seen through a strange perspective, rather than a self-enclosed world of its own.

Mr. Money-Kyrle's overt aim is not to explore the cross-implications of psychoanalytic and other thought, but to systematise the views of one segment of the psychoanalytic movement, the Kleinian, which he considers to be the most progressive. His conception of what such systematisation involves, however, is so broad, including as it does views on ethics, aesthetics and politics, that in practice his discussions, whether one agrees with them or not, do have the effect of keeping the diverse fields sensitive and alive to each other, rather than insulated and coexisting into an insensitive eclecticism.

His approach in this book is guided by a certain schema. One can see man in three ways: as an object, as the natural sciences see him, or dualistically, as common sense sees him, or finally as central and pivotal to the world which his mind had 'built', as he is seen by philosophers such as Hume and Mach. Though he also explores the first two visions, he explicitly commends the last '... as psychologists, we ought, perhaps, to reverse the order (of common sense) and think first of experience as that out of which each individual builds his world ... and I suggest that psycho-analysis could make use of it also to construct a more comprehensive picture of man in relation to his world.' This is a truly amazing marriage of psychoanalytic and epistemological thought: the egocentric predicament of the empiricist

picture of our knowledge is married to a Kleinian subjection to our infancy. This might seem a sad fate indeed, but Mr. Money-Kyrle's book is not a pessimistic one.

I wonder, however, whether the psychoanalytic and epistemological approaches really will mix well: I had always supposed that what gave psychoanalytic thought its vitality was precisely that, unlike academic psychology, it did not originate from epistemological preoccupations. It was never a theory of knowledge driving itself into the laboratory from a sense of obligation to its own principles.

Mr. Money-Kyrle's main reason for valuing the epistemological, subjective approach is that in seeing how the individual constructs his world, we at the same time see the general features of the world, of which the other sciences supply the detail features. '... the task of psychology is to describe how world-models are constructed, that of other sciences is to extend and refine specific parts of them ...' Man being pivotal and the 'constructor' of his world, the study of man is also the study of the basic construction of that world: psychology is also an *ex officio* cosmology.

This has often been the aim of theories of knowledge: by as it were catching the world as it passes through the needle's eye of knowledge, to gain a short cut to its general features. What is novel in Mr. Money-Kyrle's thought is that *three* (and not, as is more common, two) things are fused into one: a genetic account of the growth of our view of the world, a philosophical theory of knowledge which cannot but, at least tacitly, assess the validity of what we claim to know, and finally a synthesis of the findings of the sciences about the world. (One should add, in fairness to him, that he explicitly disclaims the ability to complete such a synthesis — but it is interesting to see that, on his view, a completion of a psychoanalytic system of thought would also be a unification of science: by knowing how our knowledge grows we would also know the general features of its content.) The problems which this triple identification raises are enormous, and whether solved or not, it is very interesting to see them raised. In practice, psychoanalytic thinkers do tend to see the world through their theories of what happens to the psyche, and it is good to see the problems inherent in this view brought into the open.

It is impossible to follow up all of Mr. Money-Kyrle's applications of his programme, but it is worth commenting on some. He is familiar with the grave objections to psychoanalysis, best argued by Professor K. Popper, that a doctrine so elastic that *nothing* could falsify it is of

no scientific value. Unlike some who are wedded to psychoanalysis, this point does not cause him to burst into a defensive anger. He argues with it earnestly, and his argument takes the form both of a theory of how in general we know about the thoughts and feelings of other people, and how in particular psychoanalytic interpretations come to be accepted and rejected.

Concerning knowledge of other minds in general, it is odd that he should not consider the theories of recent philosophers, who have devoted so much of their minute labours to this problem. 'Psychoanalytic reasoning, he says, is in essence very simple and of a type on which all our beliefs about other people are ultimately based. If we see two people embrace, we imagine them to be in love because we are acquainted with such feelings in ourselves.' Could we then never comprehend what we have not experienced? Or is it a part of his doctrine that in some sense we do experience all feelings our language can describe? I can hardly be accused of being a wholly uncritical follower of Wittgenstein and Ryle, but I am surprised to see him subscribe so flatly to the 'analogy with ourselves' theory of our knowledge of others, without even attempting to answer those who have claimed so firmly to have destroyed this theory.

The second part of his argument here, an account of the specifically psychoanalytic mode of choosing and rejecting interpretations of the minds of others, is, if I understand rightly, open to other objections. His account of how the analyst comes to *reject* an interpretation — and this is crucial in answering a demand for falsifiability — is in terms of the analyst's capacity to detect his own motivation which had led him to see the situation falsely. He detects falsehood by unmasking his own motives for embracing it. But this leads to a view which I suspect is deeply embedded in psychoanalytic thinking, namely, that error is always somehow motivated. Identify and neutralise the motive, error will disappear, and truth is — the remaining residue. But is this so? Is truth manifest and simply available even for a pure heart? I doubt it. Here Mr. Money-Kyrle might well consider another objection arising from Popper's thought — namely that truth is *not* manifest, for anyone, not even for those who know themselves.

Finally, a word about the author's views on society and history. They are 'psychologistic' in the extreme: the crucial factor in social changes appears to be the personality types (and their distribution in various social classes) of the participants. Mr. Money-Kyrle

distinguishes between technical change, which he believes to be 'linear', i.e. continuously progressive, and social change, which is sadly 'circular'. For one constellation of our psyches produces revolution, which brings forth another constellation and reaction. And so on. But is there no hope? May we not break out of this circle?

There is some. Psychological insight may soften and enlighten both rebels and reactionaries. Mr. Money-Kyrle feels diffident about whether this cross-bench or cross-barricades diffusion of insight can be attained, but confident about the beneficent consequences of its achievement. Social change too would then become 'linear', though he does not tell us in which direction the line would soar. The actual description given of the happy post-insight period makes it sound rather like a stable equilibrium, a balance between equality and its absence. It appears that this balance would not be the same in all places and circumstances. But what matters is that each side would be 'fully conscious of the sources of tension between them, and (would be) able to form true pictures of themselves in relation to the other'. Thus an insightful class would be happy even with tensions: or perhaps, a *very* insightful class with *very* mild tensions?

In connection with this, Mr. Money-Kyrle comments on the present international and ideological situation. Nowhere does his psychocentrism come out more clearly. '... if ... Communist autocracy had been allowed to dominate the world, most of the psychological knowledge we have so painfully acquired would have been lost ...' Thus the essence of the cold war is to preserve Freud for humanity. We are right to risk nuclear extinction in order to preserve those insights whose diffusion might eventually liberate us from 'cyclical' history. All this follows from the author's premisses, and it is good to see him have the courage of his convictions. But what if the Russians abjured Pavlov and embraced Freud? Would CND be vindicated?

This is not clear. Mr. Money-Kyrle is sceptical of 'a desire for truth arising spontaneously at the pyramid of Communist power'. To believe in this 'might be to embrace [a] lethal delusion'. Would it reassure him (or perhaps he knows) that a cautious and partial rehabilitation of Freud has appeared in the Soviet Journal of Psychology? (Of the *young* and more materialist Freud, naturally. The East prefers its Freud young and Marx old, unlike our own metaphysical leftists who like their Freud old and Marx young.) Perhaps this would not reassure him much, for the valued insight 'can only partially be communicated by books and lectures'. More of it, but still not enough, was

communicated by Freud to his disciples and by them to each other. '... great difficulties will have to be overcome before this knowledge becomes deep enough and wide enough to have much effect on the ... social cycles ...'

So, we must defend our society against the insightless barbarians, for they would destroy the knowledge which the Guardians amongst us communicate to each other, and we must hope that they multiply fast enough to be able in the end to save us from our own fatality.

Ernest Gellner

Raymond Aron, *Introduction to the Philosophy of History*. London 1961. Weidenfeld and Nicolson. 36s.

This book was first published in France in 1938. It provides the background against which the doctrinal pages of the author's post-war work 'The Opium of the Intellectuals' may be better understood. The book under review is one of the fruits of the meeting of that brilliant generation of French intellectuals that grew up to maturity in the 1930's whilst under the influence of German thought. That generation was profoundly influenced by phenomenology, although, unlike their German mentors, the French phenomenologists rapidly developed a home-grown existentialism highly receptive to certain phases of Hegelian thought. The impact of the war upon this generation led many of them from Hegel to the young Marx, partly as a result of their political sympathies and partly as a result of the inner logic of ideas. The resulting syndrome is well-known: the phenomenologists and existentialists who are also 'marxisants' of various hues, like Sartre and Merleau-Ponty.

Professor Aron was a member of that generation, sharing in its phenomenological conversion although specializing in historical and sociological questions. In the 'Opium of the Intellectuals' he attacked the views on the philosophy of history developed by his fellow-phenomenologists who were also 'marxisants'. In the present work we can see that his disagreement had its roots in a fully worked-out position long before the post-war political conjuncture. The book's discussions primarily centre round the view of great German social philosophers such as Max Weber and Simmel, and secondarily round the view of the so-called French school of sociology (Durkheim *et al.*). Although the book was revised in 1948, there are no glimpses of ideas outside a Franco-German cultural frame of reference. Indeed, the important views on social and historical philosophy of the Vienna school of neoliberals, like Hayek or von Mises, to whom Professor Popper stands very close, are not even adumbrated in the book.

The book is both esoteric and parochial. It is esoteric in that a full understanding of the views discussed in the book presupposes more than a nodding acquaintance with the thought of the German thinkers who are constantly invoked. Aron had published a full-length expository and critical work dealing with these German thinkers. The present English translation provides us with one member of a diptych rather than with a self-contained work. The book is parochial in that only certain French and German thinkers are dealt with. Aron's sojourn in England does not seem to have increased his appreciation of Anglo-Saxon thought.

For Aron, the mark of human history is man's consciousness of having a past. It is historical consciousness that makes man an historical being. Following this line of thought, he distinguishes between causal explanation and 'understanding', viz. the grasping of the meanings, intended or unintended, of man's being, deeds and works. The distinction is of course classic in German thought. Concerning the understanding of man's actions Aron draws a distinction between two types of interpretation. Firstly, there is rational interpretation, either in terms of the adjustment of means to ends as in the case of economic behaviour, or in terms of the rules of logic or style, as in the case of consecutive thought or aesthetic composition. Secondly, there is psychological interpretation, in terms of the press of impulses ranging from uncalculated but episodic reactions, as in the case of a man sweating with fright to master-passions like a Napoleonic lust for power. Aron argues that rational interpretation of human action is logically primary — notice how we call the second sort of behaviour *irrational* behaviour, as if it were a residual category — and historically primary, for the second type of interpretation tends to slur over the distinguishing features of historical phenomena (for instance, we can only too easily 'explain' all revolutions in terms of 'resentment'). This thesis of the primacy of rational interpretation is similar to if not identical with Popper's contention that the explanation of human behaviour should be essayed in terms of the logic of the situation (roughly, Aron's rational interpretation) rather than in terms of individual motives (Aron's psychological interpretation.)

The core of the book, however, is to be found in Section II at pages 86 to 156. Here Aron tries to show that the diversity of historical accounts and interpretations is not merely *de facto* but also *de jure* or rationally justified. Historical accounts necessarily depend on the historian's theoretical preconceptions as well as on the historian's 'perspective'.

Concerning the dependence of historiography upon theory Aron

points out that even such trivial terms as 'battle' are theoretical constructs, conceptual tools, not concrete entities. This is quite apart from the further impingement of theoretical preconceptions upon the selection and interpretation of the facts. Again, the imputation of reasons to people's acts is vitiated by uncertainty as to the calculations actually performed by the actors. The imputation of motives is beset by ambiguity, and can never transcend the riddle of character and personality. The exposition of the great systems of thought is always a compromise between the attempt to grasp the meaning actually intended by the thinker (which is difficult enough) and the attempt to grasp the impersonal meaning of the thoughts and their logical structure. It is precisely the great masterpieces of thought or art which unceasingly command admiration and challenge interpretation (precisely analogous considerations apply to art). Aron sums up by saying that the meanings of man's actions and works are inherently 'ambiguous and inexhaustible'.

Aron's discussion has the great merit, rare in books of this kind, of distinguishing clearly between actions, works (i.e. science, philosophy, art) and persons, and of bringing out the somewhat different issues they raise in relation to the problems of historical objectivity. This is in line with the sociologists' categories of *social* structure and change (sustained by the rational or irrational behaviour of the actors), *cultural* objects such as belief-systems and value-systems, and personalities. Aron is at pains to show the variations in the coefficient of subjectivity both as between different categories and within these categories; for instance, he shows the greater subjectivity pertaining to the interpretation of art-works as against scientific theories. Yet I fear that the total blurred impression that his discussion may leave with some is that 'it all depends' (on one's point of view). Those who profess a lazy 'vulgar relativism' ('everything is relative or subjective') may feel that they can draw aid and comfort from this work. They would be wrong, of course, for Aron does bring out not only the limits but also the possibilities of objectivity in historical thought and discourse. For instance he points out that 'logically, the narration, which in its intelligible texture is the work of the historian, is, nevertheless, valid for all' (p. 113). Again, as was pointed out above, Aron asserts that the rational interpretation of human action is privileged (p. 109) which presumably means that, other things being equal, it is to be preferred. Aron's discussion is so compact and sometimes so cryptic that it is liable to be misread as a contribution to 'vulgar relativism'.

Historiography necessarily depends, not only on the theory or the cries of the historian but also on the historian's 'perspective' i.e. the way in which he orders the past with reference to the present. To see the past in perspective has often meant, of course, that the historian saw the past as a goalward flux, a directed process towards the achievements or miseries of the present. The historian's scheme of values, anyway, determines the criteria of relevance and importance which he uses in the selection and serial arrangement of events.

Aron argues that neither the view of history as a goalward flux nor the view of history as a goalless drift, can lay claim to final truth. Cumulativeness is of course, both a fact and a value. It is a fact, although different areas of life show very different rates of cumulativeness, most striking, of course, in the case of science and technology, rather doubtful in the case of art, although by no means absent. Cumulativeness is a value, for we require that history be meaningful rather than a mere jumble of happenings.

Secular views of history as a unitary, directed process have flourished in the last two centuries. Such views presuppose that there is a single standard of values which it is man's vocation to realize, and that the meaning of history lies precisely in mankind's gropings towards the realization of this standard of value. Professor Aron shows how linear philosophies of history logically presuppose a philosophic doctrine of man: this demonstration is one of the best parts of the book. Perhaps he could have added that such linear views of history function as modern man's theodicy. The secular solution to the problem of evil, of man's suffering, is found in terms of an historical scheme of trials with an assured promise of redemption and salvation now imminent.

Professor Aron's book is long and compact, and it is positively bursting with manifold insights. The book is indeed, misleadingly called an 'introduction to the philosophy of history': in scope it is rather encyclopaedic. Yet problems like that of 'cultural relativity' are disposed of in a few pages. The reader is overwhelmed by a bombardment of ideas which yet fails to carry conviction at many points. It would have been better if the author had tried to argue with the reader instead of delivering an immensely brilliant lecture at a breath-taking pace.

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CAN KNOWLEDGE BE REACHED?*

by

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There is no amount or quality of evidence such that if that amount or quality is reached, then truth is reached. If, therefore, a proposition must be true in order to constitute knowledge, knowledge is never reached. If certain standards of evidence are satisfied I have the right to *say* 'I know', and the right does not depend on how one answers the question whether it is right *what* I say.

The conclusion of this paper will be that knowledge cannot be reached. Or rather: that in an important sense of 'knowledge' and of 'reaching', knowledge is not something which we can reach. We may already possess it — and I do consider both myself and others to know quite a lot — but I cannot see that I am able to offer a philosophically satisfactory account of how we could possibly have come nearer to it until a definite event happened: we reached it.

Formulated and interpreted with care, I consider my conclusion both non-trivial and true. It is restricted, though, to those concepts of knowledge which require that in order to reach knowledge, we must reach truth, and restricted also to those concepts of truth which require that in order to be true what we say must be so.¹

There is an old and distinguished family of definitions or criteria of knowledge with a requirement that for something to be known, it must be true, and for something to be true, it must be the case. The truth requirement may be thus formulated: Something cannot both be known to be so, and yet not be so.

The expressions 'to be so', 'to be the case', and 'to be true' are very near in meaning and function in the context relevant to our problem. If we substitute 'to be true' for 'to be so' in our formulation of the truth requirement, we arrive at a rather modest claim, closely related to

* Lecture delivered at Oxford University, October 1960. Some changes have been made in the MS.

the truth requirement: Something cannot both be known to be true and yet not be true. I mention this in order to stress that giving up the truth requirement of knowledge would lead to paradoxical, or at least very queer, results.

Within this family of definitions, or criteria, or sets of necessary and sufficient conditions, of knowledge, there is a sub-family with three requirements: one can only be said to *know* that p if, first, one is sure that p; second, one has adequate grounds for being sure that p; and third, p is true.

The requirements may be transformed into a questionnaire of three questions. Some such questionnaires cannot easily be avoided if the definitions or criteria are to be used in concrete cases by persons who want to make sure that they themselves or others *know* that such and such is the case, and not only believe it, assume it, etc.

Suppose I take it for a fact that Mr. Nixon's opinions concerning the Chinese off-shore islands are about the same as those of President Kennedy. Suppose also that in Mr. Nixon's behaviour I find indications that he himself either *knows* this to be the case, or that he has *made a guess* to that effect, or that for some reason or other, he has chosen to perform some of his actions on *the assumption* that this is the case, etc. If then I want to answer the question whether Mr. Nixon knows, or makes a guess, or assumes, etc., that so-and-so, I may decide to make use of the following kind of questionnaire:

- (1) (1) Is he sure that p?
(2) Does he have adequate grounds for being sure that p?
(3) Is p true?

In this questionnaire the third question is different from the conjunction of (1) and (2).

But now we come to the problem children of our distinguished family, the criteria, that I know that p:

- (2) 'I know that p' — 'I am sure that p, I have adequate grounds for being sure that p, p is true'.

In asking myself, or being asked by others, whether I know p to be the case, what am I to do when confronted with the third requirement, that p be true? If I am trying to decide whether *I* know or don't know p to be the case, what, then, is the relation between the first two requirements and the third one? Is it really a new question?

For the sake of an easy survey I write out both the third person questionnaire, where I answer questions about a third person, and the first person questionnaire, where I answer questions about myself:

(3) Question 1: Is he sure that p ?

Question 2: Does he have adequate grounds for being sure that p ?

Question 3: Is p true?

(4) Question 1: Am I sure that p ?

Question 2: Do I have adequate grounds for being sure that p ?

Question 3: Is p true?

There is some sort of incongruency between the two questionnaires. In deciding whether *he* knows, it is *I* who answer the question whether p is true, and, of course, I do it on the basis of my own beliefs and grounds — to which there is no reference in the questionnaire. The third person questionnaire is that of a bystander or an observer. Or it is that of an editor of an encyclopedia of knowledge, like for example Otto Neurath, whose views about knowledge, according to Bertrand Russell, were characteristic of an editor who is not also a contributor.

Faced with question 3 of the first person questionnaire, 'Is p true?', I shall once more ask myself, 'Do I have adequate grounds for being sure that p ?' — or I shall frantically search for something more than evidence, say a guarantee of truth. (Sometimes I firmly believe that I have a guarantee, but, speaking generally, I adhere to the view that there can be no guarantee for the truth of a statement whose negation is not some sort of a contradiction.)

If I have already answered 'Yes' to the question about adequate grounds, the question 'Is p true?' will either be interpreted as a repetition of the question about adequate grounds, or it will be interpreted as unanswerable or awkward, like the following question: 'Irrespective of adequate grounds and my own conviction — is p true?'

Non-philosophers, and perhaps philosophers too, in their everyday life, employ concepts of truth that are completely independent of requirements about being sure, and completely independent also of requirements about adequate grounds or the right to be sure. I have called such concepts, or conceptions, of truth 'homifugal', and contrasted them with what I have called 'homipetal' conceptions. In the homifugal conceptions there is no reference to human conditions or human activities.

Not without a struggle with my own inclinations, have I long accepted a homifugal conception of truth as inevitable. But on a homifugal interpretation, the first person questionnaire is a bad questionnaire, since the third question cannot then be answered. That is, it cannot be answered on the basis of rational considerations.

If, on the other hand, we accept a homipetal conception of truth, and interpret the third question accordingly, the question will be redundant, since the answer to it will follow from the answer to question 1 or 2.

Thus it seems that for both conceptions of truth the questionnaire is ill conceived.

Before I continue, I ought perhaps to mention how I interpret 'adequate grounds' and 'the right to be sure'. According to one interpretation, grounds are adequate only if they guarantee truth, and nothing less than such a guarantee gives you the right to be sure. This is not how I use these expressions in this paper. According to another interpretation, which is also how I use these expressions, grounds are adequate if definite, but variable, standards of evidence are satisfied, and when they are satisfied, you have also the right to be sure.

I shall now put forth a theory or an explanation, or at least a few comments, as to *why* the first person questionnaire is felt to be so awkward. Some persons have found my explanation highly speculative and metaphysical, while others have found it simple and commonsensical.

In saying 'p is true' I do not intend, or very often do not intend, to express anything that has to do with *my* relation to p. I just tell that *it is so*, that what p says *is the case*. I enjoy a homifugal attitude or bent of mind. Or, if I am a staunch ontologist, I shall say that I am absorbed in the world of objects. At least these are the words that seem to fit in retrospection.

But I am never safe in my enjoyment of truth. A shift may occur, at any moment, which makes me see, or search for, *my* relation to truth, or *my* relation to p. Of what kind is my access to truth, that I can utter, as a rational being and with a sober voice, 'p is true'? I want to get a picture of myself grasping truth and holding it in my hands.

Whether or not such reflection results in reassuring myself that p is true, the natural, and adequate, expressions of my reflection are homipetal, not homifugal. I may feel more sure than ever that Francis Bacon did not write Hamlet, that is, I am more positive than ever

in my answer to question 1; or more than ever do I consider my grounds as adequate and my methodology unassailable — and in saying so, I repeat my answer to question 2. But I shall not make use of the expression 'p is true', or any other homifugal expression, because my attention is egocentric. Perhaps, instead of answering question 3, I shall say, 'I am more convinced than ever that p is true'. But from that it does not follow that I am willing to say simply, p is true'.

There is, in some sense, no way back from the evidence attitude to the attitude of simply saying what is. On the other hand, the simple attitude of asserting truth does not have to lead to questions of evidence.

So far, I have only discussed the awkwardness of first asking for evidence and then — immediately afterwards — for truth. Maybe a simple change, asking for truth first and then for evidence, will save us from trouble. Professor Chisholm, in his book on perception, places the truth requirement as the third one. But Professor Ayer, in *The Problem of Knowledge* (Pelican edition, p. 35), places the truth requirement as the first one. He writes:

I conclude then that the necessary and sufficient conditions for knowing that something is the case are first that what one is said to know be true, secondly that one be sure of it, and thirdly that one should have the right to be sure.

Reformulated, this piece of text may well be taken as a member of the family of definitions under consideration. A corresponding questionnaire in the first person would run as follows:

- (5) Question 1: Is what I say I know true?
Question 2: Am I sure of what I say I know?
Question 3: Do I have the right to be sure of what I say I know?

For the present purpose, these questions may be reformulated as follows:

- (6) Question 1: Is p true?
Question 2: Am I sure that p ?
Question 3: Do I have the right to be sure that p ?

As an oral questionnaire this one is likely to be more successful than the previous first person questionnaires, because the subject is asked about p before he is asked about his relation to p.

Suppose I answer in the positive to all three questions, but then get worried about the first. And suppose I change my answer to the first

question from a simple 'Yes, it is' to 'I am sure, and I have the right to be sure, that p is true'. Such a change would seem to me to be irrational, that is, I cannot see that I could give it a rational justification. Any such justification would amount to a rational reconstruction of the transition from truth attitude to evidence attitude.

It seems that saying, of a given p, 'It is so', 'It is the case', 'It is true', or simply asserting p, is an act for which there can be no adequate rational reconstruction. Saying 'It is so', etc., does not entail, and is not entailed by, any statement about adequacy of grounds or firmness of conviction. Therefore, accumulation of evidence makes me say, 'There are now more grounds than ever for taking p to be true', or, 'I am now more justified than ever in my conviction that p is true', rather than repeat 'It is so'. As long as my attention is focussed on the increasing evidence, or as long as I am not absorbed in immediate perception or intuition, my verdict is adequately expressed in homipetal terms only.

The transition from truth to evidence, or vice versa, might perhaps be understood in terms of levels.

If I enjoy truth while absorbed in the amassing of evidence, my use of the expression 'It is true' refers to second order statements: 'That the weight of my evidence is crushing, is as simple a truth as can be', 'My conviction is total. That is the simple truth', 'You earned your right to be sure that the gun was unloaded, that is true. But, God bless your soul, you were mistaken in saying you knew'.

If reflections enter the scene while I am enjoying truths of the second level, the homifugal terms will disappear from that level, and if they reappear, it will be at the third level. And so on.

At any level, a philosopher may point to something I assert, enjoying truth, and correctly comment that I believe I have knowledge. But if the philosopher is inclined to argue in favour of scepticism, he may point instead to the transition from enjoying truth to collecting evidence, and correctly comment that no volume of evidence will regain for me my first enjoyment of truth.

One position that might be called sceptical, but which I do not think ought to be so called without qualifications, can be thus formulated: *No evidence of a proposition is such that if it is available then the proposition is true.*

If to reach truth, or to grasp truth, is to reach or grasp with a guarantee that it is not falsity, we can say: *Truth or falsity cannot be reached by increasing evidence.*

And if 'knowledge' is defined in such a way that 'to know that p ' implies ' p is true', then we get another set of formulations characterizing the position: *No evidence that p is so strong that if a person has that evidence, he knows that p .* In other words: *Knowledge cannot be reached by increasing evidence.*²

We might continue: If to understand or to see or remember that p is the case implies that p is true, then understanding or seeing or remembering that p cannot be brought about by increasing the evidence.

After all the talk about our not being able to reach so-and-so, we may feel better if we contemplate for a moment a case of our actually being able to reach something. I choose the case of reaching for an apple. In reaching out for it, my hand is brought continuously nearer to the apple. The act is brought to its happy end as I grasp it, and my grasping the apple is an event that stands out as something very different from my hand coming close to it. This difference is clear whether the 'apple' reached subsequently is found to be of wood and therefore not to be an apple at all.

If I am too eager, or too drunk, I may overreach myself and grasp somewhere beyond the apple. There is nothing of all this in our groping for knowledge. We can perhaps estimate, or even measure, increase in evidence, but not approximation to knowledge. The event of grasping is not forthcoming, and nothing corresponds to reaching beyond. One can even measure the distance of the hand from the apple. At a certain point, point zero, the hand reaches the apple. One can also specify an increase in evidence and perhaps find a scale for measuring this, but one cannot specify the distance from truth and the reaching of truth. One cannot fix a point zero in terms of evidence.

I have used the expression 'truth attitude' a couple of times, meaning the attitude, or state of mind, of simply enjoying truths and asserting them once in a while; in contrast to the attitude, or state of mind, of collecting evidence. Now, it seems that I can say 'A is now enjoying truth' only about others. There is something queer about the statement 'I am now enjoying truth', since it seems to presuppose reflection inconsistent with the very attitude it expresses. I say that I am at a level I cannot possibly be at at the moment I say it, or think it. My friend may say to me, 'You are now enjoying truth at level zero'. If I answer, 'Yes I am', he can say, 'No, you are not; you are enjoying it at level one'. Meekly I admit it is as he says. But only to be told that I am now at level two. And so forth.

This conversation need not worry us, however. It only reflects the fact that the truth attitude is linked to the act of asserting, and does not disappear when reflection begins. It continues, but at level $n + 1$.

That knowledge cannot be reached is of little importance if it is due to the circumstance that we already have it. But if we already have it, how are we to account for our strivings to increase evidence? Our situation seems to resemble the lot of the human race according to a certain doctrine of grace: good actions do not qualify for salvation, but they should be undertaken, and with salvation in view.

But, after all, our epistemological situation is not so strange. We strive to increase evidence because we think that we have often been mistaken, and most often when the evidence has been meagre.

The successive editions of an encyclopedia of statements satisfying certain standards of evidence may rationally be expected to show fewer corrections the higher the standards. What would be the difference between an encyclopedia claiming knowledge and an encyclopedia claiming evidence only? It seems to me that there would be none, except in the subtitle and in the accounts of how the present edition differs from the foregoing. In the encyclopedia of knowledge there will be more acknowledgements of mistakes.

It is sometimes said that the sceptic insists on standards of evidence which it is impossible to reach. If this is characteristic of all forms of scepticism, then the view I advocate is not a form of scepticism. The requirement of truth is independent of the requirement of evidence. I do not quarrel with the standards of evidence; they have, perhaps as a result of the prestige of the natural sciences, been placed too high in many fields of inquiry.

But if we do not make impossible standards of evidence part of our definition of scepticism, which, of course, we ought not to do, then the view I advocate is, I think, a form of scepticism — since it asserts the unattainability of knowledge. Whether it is a trivial form or not, is a question we might cleave in two: one psychological and one logical. To the psychological question I can only answer that, to me, it is not a trivial form: I cannot but feel sorry and deceived, having imagined that in our search, and in particular in our research, there is a guarantee that we shall reach truth, and therefore knowledge. I can still *imagine* that the conditions of human existence were such that in our strivings we would sometimes reach knowledge, and, of course, know that we had reached it. But I cannot imagine the conditions themselves.

Perhaps the *meaningfulness* I attach to my form of scepticism cannot

be given a rational expression, with propositional content and logical consequences. If it cannot, then the answer to the logical question is not only that my form of scepticism is trivial, but that it is logically non-existent. But I think it can. Even if I find it impossible to describe the conditions I imagine possible, and so must be content with a set of negations, like 'Knowledge cannot be reached by increasing evidence', and a set of rules limiting the applicability of such a maxim (that it must not include itself, etc.), such maxims are themselves meaningful propositions with consequences that can be derived by the usual rules of inference.

NOTES

- ¹ Another restriction is this: I do not speak about 'knowledge' that this is a table, that it is not raining on the table, etc. With certain kinds of everyday utterances, we scarcely talk about evidence, collecting more evidence, etc. I am talking about utterances in relation to which it is pertinent to ask, 'What about the evidence?'
- ² This last maxim does not, of course, hold if one takes 'p is known' to be implied by 'our evidence that p is up to the relevant standards'. But then one must also face the possibility of having to accept statements such as this one: 'Yesterday Mr. A knew that there are birds on Mars whereas Mr. B knew that there are no birds on Mars'.

DESCRIPTIVE ANALYSIS AND THE SCEPTIC

A Critical Appraisal of A. J. Ayer's
The Problem of Knowledge

by

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Abstract: On the basis of Professor Ayer's *The Problem of Knowledge*, Mr. Stigen criticizes Ayer for defending a position which the sceptic does not attack. Ayer's "descriptive analysis", which is his answer to the sceptic, consists in an analysis of statements about, e.g. material objects or other minds into verifiable propositions. In other words, Ayer points to the fact that our statements are shown to be true by verification. However, according to Stigen, this analysis does not remove the sceptic's doubts, for the sceptic does not doubt that our statements are true; his attack is directed against the validity of those of my present beliefs for which I claim knowledge-status. The sceptic asks me to justify my claim that the statement is not only true, but that I have also good grounds now for being sure of it, i.e. that my present belief amounts to knowledge in virtue of its credibility. On this point, according to Stigen, Ayer offers no satisfactory solution.

In 1812, as the present writer observed to him [sc. Dr. Thomas Brown] that Reid and Hume differed more in words than in opinion, he answered, "Yes, Reid bawled out, We must believe an outward world; but added in a whisper, we can give no reason for our belief. Hume cries out, We can give no reason for such a notion; and whispers, I own we cannot get rid of it."

Sir James Mackintosh, *Dissertation on the Progress of Ethical Philosophy, Chiefly during the Seventeenth and Eighteenth Centuries* (2. ed.; Edinburgh, 1837), p. 346, note.

Philosophy has changed greatly during the twenty years since Professor Ayer wrote his first book, *Language, Truth and Logic*, and Ayer himself has contributed significantly to that development. Ayer's latest book¹ bears witness to the new philosophical climate, and if anything is to be judged from a comparison between his first and his latest book, the change has been all for the good. It is, let us hope, symptomatic of the nature of present-day philosophical discussion — as against that of the thirties — that Ayer has become more sensitive to the complexity of philosophical problems and more hesitant to propose sweeping solutions; in fact, he warns us that "the way in

which [philosophical questions] are answered" (1)² may itself determine the solutions we give.

Such a view of philosophical activity is particularly important in the discussion of a subject of the kind Ayer has chosen for his book — the sceptic's objections to the possibility of knowledge. For as the discussion proceeds, it emerges, as Ayer indeed points out from the beginning, that "the philosophical importance [of the sceptic's objections] comes out in the discussion of *what lies behind them*" (28), that the sceptic's paradoxes result from the way in which he asks his questions; although we are right in rejecting the sceptic's conclusions as wrong, or even "perverse", we must discover precisely where he goes wrong, and this can only be done by a consideration of "the *arguments* by which it [i.e. the sceptic's attack] is supported" (33). This seems a much more fruitful approach to scepticism than the other two attitudes manifested in recent British philosophy: to reject the sceptic's conclusions because they are not *true*, without bothering to analyse his argument, as did G. E. Moore,³ or to embrace the sceptic's arguments because they are "*logically* impeccable", as Bertrand Russell⁴ has sometimes been apt to do. Instead of fastening on truth or logic, Ayer has chosen as his fixed point the first of the three topics suggested in the title of his first book — *language*, in terms of which both truth and logic may be discussed. This ought to have made him more sensitive than either Moore or Russell to the decisive role which reasoned discourse itself plays in the formation of the views taken of complex philosophical problems. "In all philosophy so much depends upon the *way* in which things are put" (28); in philosophy, as elsewhere, it is a mistake to suppose that language can mirror completely the object to be investigated, to seek "as it were to merge their language with the facts it was supposed to picture", forgetting that the linguistic signs are "symbols", not mirrors, and expecting them to have "the solidity which belongs to the facts themselves" (55).

The main pattern of the work is easily discernible. The first chapter — entitled "Philosophy and Knowledge" — serves to determine the subject to be discussed in the rest of the book. It ends with a definition of 'knowing', but it is a definition of a peculiar kind, as we shall presently see, and its function is to delimit roughly the topic to be treated rather than to say anything about its nature. Chapter 2 (sects. i, ix and x) raises the particular issues within the topic thus delimited, and the rest of the book discusses and sets forth various answers on these issues — the particular problems of the theory of knowledge.

I. Determining the topic

The discussion of knowledge given in the first chapter ends with a specification of the conditions for *knowing* that something is the case. Ayer himself calls it a "definition of knowledge" (34), and there is no point in disputing his terminology. It should be pointed out, however, that the definition serves to *identify* the object to be investigated rather than to *describe* the results of that investigation. It is rather like pointing than describing, as indeed all determination of the meanings of terms by reference to the surroundings, the circumstances, the context or the 'conditions' seems bound to be. The necessary and sufficient conditions for knowing that something is the case are the following three: (i) that what one is said to know be true, (ii) that one be sure of it, and (iii) that one should have the right to be sure (34).⁵ What is original in this definition is the formulation of the third condition. Whereas the traditional definitions of knowledge state that one must be able to give 'evidence' for being sure of what one knows, Ayer does not want to include in the definition any mention of the kind of conditions which, if fulfilled, would give us a right to be sure. "This right may be earned in various ways; but even if one could give a complete description of them it would be a mistake to try to build it into the definition of knowledge, just as it would be a mistake to try to incorporate our actual standards of goodness into a definition of good" (34).

It is reasonably clear why Ayer does not want to include these standards in the definition, for that might be thought to beg precisely the questions which are to be raised by the sceptic and discussed in the rest of the book. Moreover, as we shall see, the sceptic is prepared to attack any standard that may be specified in the definition. However, if no standards are specified, he must attack 'the right to be sure' itself, and that, Ayer thinks he can show, is equal to raising an illegitimate question. The sceptic will then not only have to reject any particular claim to knowledge, but he will have to reject "the legitimacy of the title to be sure" itself and the possibility of knowledge in general (34). By defining knowledge so widely that both the sceptic and Ayer can agree to it, although the question is left open whether or not anything exists answering to it, Ayer can construe the disagreement between him and the sceptic as genuine, i.e. as a disagreement "about the *application* of the word, rather than its meaning" (33). The sceptic understands what Ayer means when he says he knows there is a pen on his desk, but he rejects Ayer's right to call it *know-*

ledge, not on the ground that it is not true, for it *is* true, nor on the ground that Ayer is not sure of it, for he *is* sure, but on the ground that he has not the right to be sure, for he cannot *possibly* have it.

Although there can be no doubt that Ayer's definition serves well to start off the discussion, the question still remains whether it is philosophically adequate, and whether it improves on the traditional definitions. The originality of Ayer's definition consists in the originality of the formulation of the third condition, serving to distinguish knowledge from true belief, viz. that a person, in knowing a proposition *q*, has "the right to be sure" that *q*. In asserting that I know *q*, I *claim* the right to be sure that *q*, and I "*vouch for the truth of*" *q* (13). In the same way, to say of some other person that he knows *q*, is to "*concede to him the right to be sure*" that *q* (31), and I "*grant that what he is said to know is true*" (14). An important consequence follows from such a conception of knowledge, for on this conception the man who knows cannot be distinguished descriptively from the man who has true belief only. As far as their state of mind is concerned, it may be exactly the same: "His procedure and his state of mind, when he is said to know what will happen, may be exactly the same as when it is said that he is only guessing" (31). "While the respective states of mind of one who knows some statement to be true and another who only believes it *may* . . . be different, it does not seem that there *need* be any difference between them when the belief is held with full conviction" (12). Therefore, to say of a person that he *knows q* is "not primarily, if at all, to describe his state of mind" (13—14); at least, "it does not *merely* describe his condition of mind" (16), and the expression 'I know' or 'He knows' cannot have primarily a descriptive use (13). The alternative is that it consists in claiming or granting a certain *excellence*⁶ or quality for the belief, and that it is the possession of such excellence which distinguishes knowledge from mere true belief. The essence of knowledge can therefore not be expressed in descriptive terms but by some kind of evaluative expression, such as "the right to".

Ayer's conception of knowledge — which thus consists essentially in viewing it as a 'virtue' or 'excellence', although he does not use these words⁷ — is a fruitful one, but it raises some difficulties, mainly because Ayer does not make it sufficiently clear to his reader precisely what *kind* of quality or excellence a belief must possess in order that I may grant it the privilege of being called a piece of knowledge. By analogies he in fact suggests that it is a moral quality. It would be a mistake,

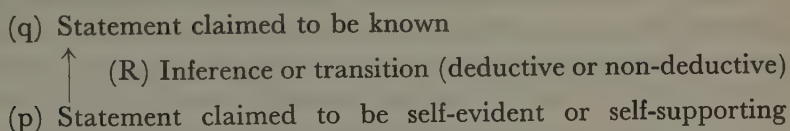
Ayer says, to specify the standards by means of which the right to be sure is conceded or claimed "just as it would be a mistake to try to incorporate our actual standards of goodness into a definition of good" (34, cf. 33).⁸ Ayer is of course aware that if 'knowledge' is not a descriptive word, the alternative is certainly not that it is used to express *moral* evaluation. But precisely *what* is the nature of the standards by which the right to be sure is conceded or claimed, he fails to make clear in the definition. And, one may ask, would not such a specification beg the sceptic's question? A specification of the standards themselves it would certainly be a mistake to incorporate into the definition. But to state the *nature* of the standards would not, I think, beg the question. On the contrary, it seems that without some assumption as to what kind of standards might count as justifying the right to be sure, no defence can be put up against the sceptic's attack.

Failure to point out that to assert 'I know q' is a claim for a *rational* quality that sets it above mere true belief, failure to draw attention to the difference between the *intellectual* excellence that adheres to the state or act of knowing, on the one hand, and the *moral* excellence adhering to behaviour, on the other, may lead people to believe that 'being sure' is dependent on our will, as are moral actions, that conceding or claiming the right to be sure is a question of choice and decision. I am not sure that Ayer does not think so himself. Discussing the justification of our right to be sure of some statement q by putting forward some other statement p which supports q, and justifying the knowledge of p in turn by some more basic statement o, and so forth, Ayer contends that at last we may "reach a statement which we are *willing* to accept without a further reason . . . At a certain point we *decide* that no further reason is required" (74). But, surely, this is not a matter of decision; I stop at a point where it is — logically or psychologically — unreasonable to ask for more evidence. We do not ask ourselves whether or not we *ought* to be sure, *ought* to claim the right or *ought* to concede it.

II. Raising the problems

Although Ayer does not want to incorporate the standards of knowledge in the definition of it, we shall have to know something more about them in order to understand precisely where the sceptic attacks, for he attacks our standards. One way of trying to discover these standards would be to "consider what would count as satisfactory answers

to the question "How do you know?" (29). To answer that question, in order to claim the right to be sure of q , we can take one of two courses: either (1) that q is "self-evident" or "directly warranted by our experience", or (2) that q is validly *derivable* from some other statement p (40). Leaving out for the moment the comparatively few cases in which we can take the first course, the second course may be diagrammatically represented as follows:



(This diagram also covers the first possibility, for to claim that q is self-evident or directly warranted is to place it on the p -level.)

To claim the right to be sure of q , is then to assert: $p \rightarrow q$ or pRq , or as Ayer (who does not use diagrams or formulae) explains it: "To give an answer [to: How do you know?] is to put forward some other statement [p] which *supports* [R] the statement of which knowledge is claimed [q]; it is implied that this second statement [p] is itself known to be true" (74).⁹ Now, if the sceptic is going to throw doubt on our grounds for accepting q as knowledge, he must consequently attack either p or R , he must argue (a) "either that the statements [p] which we take as requiring no further proof, beyond an appeal to intuition or experience, are themselves not secure," or (b) "that the methods of derivation [R] which we regard as valid may not really be so" (40). On the other hand, if we can show that p and R are known and valid, the sceptic must accept q also; that is, "the question [of what grounds there are for accepting q] is satisfactorily answered if the grounds themselves [p] are *solid* and if they provide [R] the statement [q] with *adequate support*" (74).

The sceptic's attack may thus take either of two forms: (a) Against the grounds on which we take any statement not to require any further proof. This attack is discussed in Chapter 2, sects. ii—vi. (b) Against the grounds on which we claim validly to pass from statements on one level (p) to some other level (q), i.e. against the validity of the inference from p to q . This attack is discussed in Chapter 2, sect. viii, and in Chapters 3—5. Although these two attacks are levelled at different points, they are in both cases attacks on the *value* of "the grounds on which we are normally ready to concede the right to be sure" (33)

and what the sceptic calls in question is in both cases the *adequacy* of our "standards of proof" (35). In either attack, what the sceptic does is to set forth conditions so strict that the standards used by us are seen not to put our claim to knowledge to a severe enough test. Ayer's answer consists in an attempt to show that the sceptic's conditions are too strict, that they make knowledge virtually impossible, and that unless we accept the standards that we actually use, we cannot be sure of anything. "Not that the sceptic's argument is fallacious; as usual his logic is impeccable. But his victory is empty. He robs us of certainty only by so defining it as to make it certain that it cannot be obtained" (73). Ayer's argument against the sceptic for the validity of a statement *q*, is to analyse that statement into *p* and *R*, to trace it back to what traditionally have been called 'principles from which' and 'principles according to which' *q* is derived, and to maintain that to go beyond that would involve us in a search for what cannot possibly be found. Ayer is not the first to adopt this line of defence, but in so doing he manages to discuss the problem from a great many other points of view, and this is what makes his discussion valuable and informative.

III. The sceptic's attack on the possibility of indubitable statements

The sceptic's first attack is directed at the possibility of a statement of the quality which we claim for our proposition *p*, i.e. a *statement* of such a *kind* that it is "not considered to be in *need* of any external support" (74) or of such a kind that it cannot have any support. A priori statements, the Cartesian *Cogito* and first person statements of immediate experiences have claimed such a status, and Ayer gives an illuminating discussion of these claims. His conclusion is that as *statements* they do not have the required certainty. However, instead of asking for a class of statements which are certain in virtue of the *kind* of statements they are, we may ask for the *conditions* or the *circumstances* under which certain statements do not need any support or have whatever support they need. If the question is put in this way, the answer seems to be that there are indeed such circumstances, but that the expressions which are then warranted are not genuine statements, either because they are void of descriptive content or because they are unverifiable in precisely the respect in which they claim certainty.

Thus, 'I am conscious' and 'I exist' are not descriptive, for they do not characterize any experience; the 'I' in 'I am conscious' is merely demonstrative: "It merely points to the existence of whatever it is, in the given circumstances, that makes its own use possible" (51). 'I exist' or 'I am conscious' cannot be false, but simply because it cannot be made except in *conditions* which include its validity — "because its being valid is a condition of its being made" (53). — Ayer comes to the same conclusion in respect to first person statements of immediate experience, e.g. 'I have a headache'. In so far as this is a 'statement' in Ayer's terminology, it does not include any time reference, and only the assertion 'I have a headache *now*' could be immune from doubt (58). To the extent that it is incorrigible, however, it is to be classed with "gestures" and "ejaculations", just like the *Cogito* (52). The fact that it is linguistic in itself does not raise it above the level of merely *expressing* or *registering* experiences, impressions and passions.

The distinction which Ayer applies is one between two kinds of linguistic expressions: (1) "signals" (51) which merely *register* or *recognize* one's present experiences, impressions, sensations, observations or passions without "venturing beyond" them (65, 67, 71) and which are therefore indubitable and infallible, and (2) sentences properly called "statements", which are "symbols" (55) and are informative, verifiable and descriptive, which "go beyond" the content of one's present experience and are therefore fallible and corrigible (68). This distinction is important and, I think, necessary in discussing whether or not any statement is immune from doubt. Some philosophers, among them may be Descartes, seem to have ignored it, whereas e.g. Wittgenstein in recent times has drawn attention to it as the difference between (1) linguistic expressions which refer to ("beziehen sich auf") a sensation and which are little more than "primitive, natural expressions of the sensation" and (2) sentences which "convey a meaning (bezeichnen)" and "inform".¹⁰ However, neither Wittgenstein nor Ayer throws much light on the basis on which this distinction is made, and consequently its application is also rather obscure. To keep to Ayer, how is, say, the concept of a sentence 'going beyond the content of one's present experience' related to its descriptive and informative, as against its merely expressive, demonstrative use? Why does Ayer feel that only sentences which thus venture beyond one's momentary private experience are properly called 'statements'? (58) Is it because he requires them to be of the nature of universals?

Some light might be thrown on the distinction by regarding it not primarily as differentiating two kinds of linguistic expression, but rather as differentiating two uses of language. The principle of distinction might then be found in the different *purposes* language serves in its respective use of these two kinds of expression. (1) Language can be used to identify, point to or single out for attention some object (thing or person) by means of an expression functioning as a demonstrative reference. This expression may sometimes be attached to a proposition ('The house which you see over there') or have some other form which appears to imply truth or falsity ('Our honourable guest tonight'). However, to the extent that it is intended as a reference only and is taken as such, it cannot be true or false, corrigible or incorrigible; it can only succeed or fail to do its job, identifying reference. (2) Language can be used to attribute certain characteristics to what is singled out for attention and treatment, i.e. language can be used descriptively and informatively. This use must necessarily involve comparison or contrast with other characteristics not present. In other words, the expression used 'goes beyond' its present context and thereby becomes corrigible, true or false. There seems to be only one way to avoid this: by ridding the words used in the description of all reference to the past and the future and in general to all other contexts except the present one. However, this is hardly a solution, for it amounts to giving up the descriptive and informative use of language altogether, and we are left with its referring use only. Instead of description we get naming, instead of characterisation christening, instead of predication identification. To speak of such expressions as 'incorrigible' or 'indubitable' is meaningless, and involves no ascription of excellence or basic-ness, for we could not possibly imagine how an act of naming could be corrected or doubted — although it could well fail to take effect or go wrong in some other way.

The conclusion of Ayer's discussion whether there are statements which are immune from doubt is that there is no such *kind of statements*; there are, however, *conditions* under which our claim to knowledge cannot be challenged. And this is all we need. If the sceptic asks for more than that, if he requires statements of such a kind that their being false would be logically impossible, "he robs us of certainty only by so defining it as to make it certain that it cannot be obtained" (73).

Ayer thus reaches the conclusion suggested by Aristotle in his discussion of the infallibility of immediate experiences:

The truth is not that what appears exists, but that what appears exists *for him to whom* it appears, and *when*, and *to the sense to which*, and *under the conditions under which* it appears. And if they give an account of their view, but do not give it in this way, they will soon find themselves contradicting themselves. For it is possible that the same thing may appear to be honey to the sight, but not to the taste, and that, since we have two eyes, things may not appear the same to each, if their sight is unlike. (*Metaph.* iv. 6. 1011 a 22-28., — Ross's translation.)

Contrary to Aristotle, however, Ayer does not try to state the conditions which, if fulfilled, would make it absurd or unreasonable to challenge our claim to know. This defect is a serious one, it seems to me, for it is on the nature of these conditions that disagreement arises between the sceptic and the claimant to knowledge. The failure to state the nature of the conditions under which it is no longer reasonable to ask for further grounds, is a particular case of the general failure to point out the nature of the standards by which the right to be sure is conceded or claimed, which was pointed out towards the end of section I.

IV. The sceptic's attack on the validity of inferences

However, the sceptic's main assault is still to come. For if we try to justify our knowledge of *q* by going back to *p*, we still have to show that we are justified in using *p* as evidence or support for *q*, even if we grant the truth of the "backing" or the "support". "When, as is commonly the case, a statement [*q*] is accredited on the basis of certain others [*p*], their support of it must be *genuine*; the passage [*R*] from evidence [*p*] to conclusion [*q*] must be *legitimate*" (76). It is at this point that the sceptic's main attack is directed; "he produces arguments to show that the steps which we presume to be *legitimate* are not so at all" (76). This attack challenges "our *justification* for deriving statements from one another" (40). Whereas the sceptic's first attack concerned the possibility of having *evidence* of a certain infallible kind, he now attempts to throw doubt on our justification for *using* some statement *as evidence* at all.

This attack can be directed (i) in general against *inferences* of a certain kind, or (ii) in particular against the valid derivation of certain *kinds of knowledge*. (i) The sceptic's general attack on methods of derivation could be directed either against the validity of deduction

or against the validity of non-deductive inferences. The former kind of objection is not considered at all by Ayer, the latter kind is treated rather cursorily in Chapter 2, sect. viii, where the problem of induction is discussed.

(ii) The pattern of the sceptic's attack against certain kinds of knowledge or rather against the validity of the passage (R) by which certain kinds of knowledge (q) are arrived at (from p), is set forth in two brilliant sections at the end of Chapter 2. The attack is directed against the valid derivation of four kinds of knowledge: our knowledge of (a) physical objects, (b) the past, (c) other minds, their inner thoughts and feelings, and (d) scientific entities, such as atoms and electrons. These are supposed to be derived respectively from sense-experience, present records and memories, the overt behaviour of other people, and the world of common sense. It will be seen that the derivation or transition from p to q is in these cases of a peculiar kind, for "we appear to end with statements [q] of a different category from those with which we began [p]" (81). Now, the sceptic does not want to challenge the statements with which we began, but by questioning "our right to pass from one sort of statement [p] to another [q]" (40), he throws doubt on "the validity of our belief in the existence of physical objects, or scientific entities, or the minds of others, or the past" (81). This gives rise to such philosophical questions as "whether the material world is real, whether objects continue to exist at times when they are not perceived, whether other human beings are conscious in the same sense as one is oneself" (1).

The sceptic's argument against the validity of these four kinds of knowledge implies four movements: He first tries to show that for our knowledge of q (physical objects, past events, other minds, scientific entities) we depend entirely on p (sense experiences, present memories, how other people act and talk); secondly, he shows that the relation R between p and q is not deductive; thirdly, that R is not inductive either; fourthly, and consequently, there is no justification for believing q. — It is by arguments of this kind that the sceptic performs his main service to philosophy, according to Ayer (40), for he forces us to devise "a *way of bridging or abolishing* this gap [R]" (85). The different ways of trying to do this — of trying to justify our claim that we *know* q — mark out "different schools of philosophy, or different *methods* of attacking philosophical questions" (85). These methods are four in number, and it is "interesting", Ayer remarks, "that each of them consists in denying a different step in the sceptic's

argument" (85). Naive Realism insists that we are directly acquainted with *q*, that there is no need to appeal to *p* as evidence, and that there is consequently no transition (*R*) to be justified (A difficulty: Does the naive realist insist that we are directly acquainted with atoms and electrons?). Reductionism tries to show that *q* is really of the same kind as *p*, that what we call physical objects are in fact sense experiences, etc.; thus, Reductionism insists that the *q*'s are deductions from, additions or collections of *p*'s (A difficulty: Does the Reductionist claim that atoms and electrons can be reduced to the world of common sense?). Those who take the Scientific Approach think *R* is of an inductive kind. The Method of Descriptive Analysis (which appears to be Ayer's own method) denies the fourth step in the sceptic's argument — the step to his conclusion that, since the inference from *p* to *q* is neither deductive nor inductive, it cannot be justified at all — by denying the assumption on which the step is made, viz. that there are no valid non-deductive inferences other than induction.

The pattern of the sceptic's argument — and the programme for the rest of the book — is thus clear. The success or failure of each of the four methods in dismissing the sceptic's rejection of our claim to knowledge may be discussed relatively to each of the four kinds of knowledge. Thus, in Chapter 3 ("Perception") are considered the four possible answers to the sceptic's doubt "whether the exercise of *sense-perception* [*p*] can in any circumstances whatever *afford proof* [*R*] of the existence of *physical objects* [*q*]" (36); in Chapter 4 ("Memory") the four methods are applied in an attempt to remove the sceptic's doubt "whether we can ever be justified in inferring [*R*] from present experiences [*p*] to past events [*q*]" (36); in Chapter 5 ("Myself and Others") the four methods are used to remove the sceptical doubt whether on the basis of our observations of the state of another person's body or by the things he says and does (*p*) we are justified in inferring (*R*) anything about that person's thoughts and feelings (*q*). — For reasons not stated by Ayer he does not consider the fourth kind of knowledge, the justification of our belief in (the existence of) scientific entities on the basis of "the world of common sense" (82).

On each of the three topics dealt with Ayer's final answer to the sceptic is similar in form. As we have seen, it consists in rejecting one of the sceptic's assumptions, viz. that all valid inferences must be either deductive or inductive. In answer to the sceptic the descriptive analyst admits "that the inferences which are put in question are not deductive and also that they are not inductive ... But this, it is

held, does not condemn them. They are what they are, and none the worse for that" (87). But can Ayer leave the question at that? Must he not somehow *prove* that the inferences put in question *are* valid? At this point Ayer's own assumption is brought into the open again: If knowledge of physical objects etc. is possible, then "no *justification* of these procedures is necessary or possible". Since we *do* know physical objects etc., "no more in these cases than in the case of the more general problem of induction can there be a *proof* that what we take to be good evidence really is so. And if there *cannot* be a proof, it is not sensible to demand one. The sceptic's problems are insoluble because they are *fictitious*" (87). What the problem boils down to in the end is "whether the procedures, which sustain our claim to knowledge [of kind q], do or do not require a *proof* of their *legitimacy*" (90). Ayer thinks that he can show that they do not, that there is no need for justification. What Ayer thinks he establishes, then, is that he is justified in simply *positing the assumption*. The justification of this right must, as Ayer is well aware, necessarily take the form of a non-demonstrative, dialectical proof (the classical pattern of which is given in Aristotle's proof of the principle of contradiction). Since it is the justification of an *assumption*, it must consist in showing that without it knowledge is not possible. Now, this can be done without circularity when what is at stake is the possibility of *all* knowledge, if the sceptic will only admit something, and not just wag his finger, but it is hard to see how Ayer can escape the charge of circularity when the question concerns the legitimacy of a certain *kind* of knowledge, the existence of which the sceptic does not admit.

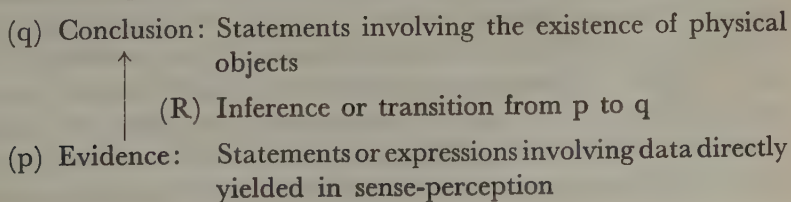
In any case, Ayer's argument is clear, although there may be doubts about its adequacy: In answer to the sceptic's doubt about the validity of our claim to know a statement q, Ayer first observes that q can be *analysed* into p and R. If knowledge of p and the validity of R are granted, valid knowledge of q follows. By his two basic assumptions Ayer is committed to this approach; any claim to knowledge of a statement q *has* to be justified by analysis in terms of "evidence" (p) and "inference" or "transition" (R). When the analysis has been completed, the argument stops, for we should then have arrived at statements p asserted *under such conditions* that the sceptic cannot *reasonably* ask for any further reasons to accept them, and as for R, one cannot *reasonably* be asked to demonstrate that the procedures which sustain our knowledge are legitimate. "It does not greatly matter", Ayer maintains, "whether we regard the need for analysis as *superseding* the demand for

justification, or whether we make the justification *consist in* the analysis" (90). In other words, what Ayer is trying to show is that "questions about the *possibility* of knowledge are to be construed as questions about the *analysis* of different types of statement and about the grounds that there may be for accepting them" (22).

Whether Ayer's argument adequately answers the sceptic's objections, can best be considered in relation to the various kinds of knowledge selected by Ayer for treatment in the rest of the book: our knowledge about physical objects, about the past, and about other minds.

V. Statements implying the existence of physical objects

The first of the three sceptical problems considered by Ayer is how to justify "our belief in the existence of the physical objects which it is commonly taken for granted that we perceive" (91). But why should we need any justification at all? This is indeed a question to be settled first of all, and in the first four sections (91—125) of Chapter 3 Ayer considers it from various points of view. This discussion serves as "the formulation of the sceptic's problem" (124) for it establishes the first step in his argument. It serves at the same time to refute the naive realist, who denies the validity of making this step. This step consists in showing that there is a gap between statements asserting or entailing the existence of physical objects (q) and some other kind of statements (p), which serve as the *evidence* for q-statements. The sceptic doubts the *legitimacy* of the transition or inference (R) by which we reach our *conclusions* (q) on the basis of what we start with (p), for the conclusions (q) seem to "go beyond" the evidence (p) on which they are assumed to depend (91). What this evidence really consists of and what the p-statements really are about, is a matter of dispute, but they are commonly regarded to be some kind of data directly yielded in sense-perception. Ayer's formulation of the problem may thus be represented in the following diagram:



The first step in the sceptic's argument is granted, and his problem must be admitted as genuine, once the difference between p and q is granted, i.e. that a distinction can be drawn between "things as they *seem*", knowledge of "how physical objects *seem*" (124), "what things *seem* to be" (125), on the one hand, and "things as they *are*", knowledge of "how physical objects *are*" (124), "what things *really are*" (125), on the other. The sceptic's problem is admitted once it is granted that there is a "difference of level" between p and q (91), that p is in some sense more basic and on a lower epistemological level than q. Still, the problem of what things p-statements are about, what is their subject-matter, haunts us, and it calls for an answer.

Ayer first considers the attempts at describing the two levels, p and q, in terms of the privacy and publicity of their objects, respectively. A physical object is "accessible, at least in theory, to more than one sense and to more than one observer", whereas the object of p, whatever it is, must be "private to a single sense" and "private to a single observer" (92). This leads to the contention that what we have on the p-level is some kind of "an idea, or an impression, or a presentation, or a sense-datum" (92) in the sense of a "mental image" (94), and to the paradoxical conclusion that what we *directly* perceive in sense-perception are images somehow in the mind. I think that Ayer to some extent misconstrues the arguments of the exponents of this theory (Ayer refers to Locke and Russell explicitly), and that to the extent that he does not misconstrue them, he himself adopts their argument. An examination of Locke's argument will show that the essential characteristic of his "simple ideas" is not their privacy, but their *simplicity*, which essentially means that they cannot be further analysed and therefore are to be taken as the fundamental elements out of which our knowledge is built up.¹¹ Similarly, the essential characteristic of Russell's "hard data" is their *hardness*,¹² which means that they cannot be divided or otherwise analysed into simpler elements. What both Locke and Russell strive to reach down to is some kind of epistemological entities which lie at the bases of our knowledge of physical objects and beneath which it is impossible or needless to go; that these entities may turn out to be private is not a requirement but a rather unfortunate consequence. But if this is what the exponents of this method are searching for, Ayer is searching for the same thing, for the statements or expressions (p) which he adduces as *evidence* or *premises* for q-statements can function in this way precisely in virtue of being on a lower epistemological level, i.e. because they

are such as cannot or need not be further analysed, since they express something directly presented to us in sensation. Moreover, the "seeming-objects" (106, 120) which Ayer finally suggests as the objects with which p-statements are concerned, seem to be just as much of the nature of mental images as Locke's ideas, Hume's impressions and Russell's hard data, and much more so than, for instance, Aristotle's objects of the special senses. In short, what they all search for is some kind of *epistemological principle* which may serve as the rock bottom of our knowledge of the external world, and Locke, Hume, Russell and Ayer all find it in ourselves. The problem then becomes: How do we move from the bottom level (p) to the physical object level (q)? This is the sceptic's problem.

What underlies Ayer's own "method of introducing sense-data" (sect. iii: pp. 104—15) is this search for something behind which I am not epistemologically required to extend my search, an entity which does not call for — or perhaps does not allow for — further analysis, something whose "existence and character . . . is not affected by the question whether the perception is veridical or delusive" (115). But the question remains why, indeed, we should engage in such a search. Its necessity and legitimacy is doubted by the naive realist. Why cannot Ayer allow the naive realist to stop with q-statements and refuse to follow him and the sceptic? Ayer's answer is that whenever someone sincerely asserts a q-statement (claims to know something about a physical object), he must somehow also be aware of *experiences* which can be expressed in p-statements: "From the statement that I see the cigarette case [q] it is . . . at least supposed to follow that it seems to me that I see something or other [p]" (105), i.e. whenever some member of a class of statements q is asserted, truly or falsely, some member of a class of statements p must be true. The naive realist is thus both psychologically compelled and logically committed to join in the dive for the epistemological bottom, according to Ayer. If he refuses, he "denies us an insight into the *analysis* of perceptual statements [q]", for we can (and therefore ought to?) analyse them into p and R. Those who stay on the surface and refuse to join in the depth-analysis fail to bring to light "important distinctions" (107) "of philosophical interest, which the ordinary methods of description [in terms of q-statements] tend to conceal" (119).

Granted, then, that we can and should analyse physical object statements (q) into sense-data statements (p) — the most conspicuous feature of which is not their privacy, not even that they are experiences,

but that they serve as evidence and ultimate premises of our knowledge of the existence of physical objects — what is the exact *relation* (R) between p and q? How are p and q *connected*, psychologically and logically, and how are they *distinguished*, psychologically and logically? This is of course precisely the main problem at stake, but two points should be noted at once. First, as already noticed, whenever some q is sincerely made, truly or falsely, it is assumed that some statement of the class p can be truly made. This relation is not a truth-relation, for the content of the q-statement does not determine the content of the p-statement. Secondly, although we are thus led psychologically to adduce some experience (p) as *evidence* whenever some statement involving the knowledge of physical objects (q) is thrown into doubt, the adduction of a sense-data statement (p) or any number of such statements does not logically entail the assertion of the existence of a physical object (q): “The expression [of the experience or the sense-data] must be understood in such a way that the existence of the physical object which appears to be referred to remains an open question: there is no *implication* either that it does exist or that it does not” (111). It should be noticed that this requirement follows from the nature of our search — a search for statements more secure than q, for if p-statements *entailed* the truth of any q-statement, the validity of p/would stand and fall together with the truth and falsity of their entailed q-statements. Sense-data statements, or statements introduced by “it seems that”, must be used “as a means of signifying how things look, or feel, or otherwise appear, irrespective of any judgement that one may be led to make about their physical existence, or of the degree of confidence with which one makes it” (112).

The sceptic’s and Ayer’s problem seems to arise from these two characteristics of p, taken in conjunction. For how can sense-data statements possibly function as *evidence* for physical object statements when they are allowed to refer “only to what is ‘visually given’ to me, *irrespective of its connection* with anything else” (108)? How can p possibly *support* q when p-statements are construed and defined in such a way that they in no way “go beyond” mere sensations?

Ayer insists that “the way that things appear [p] supplies both the *cause* of our *tendency* [R] to judge that they really are whatever it may be [q]” and even that it supplies “the *ground* for the validity of these judgements [q]” (112), but at the same time he requires that “the way that things appear” should be stated in such a manner that the statement does not point beyond itself to the way things are, they

should be considered apart from the "natural inclination" to judge how things are on the basis of their appearances (113). What Ayer requires of his sense-datum language is not only that any statement (p) within it should not *logically* entail the existence of physical objects (q). Ayer is so intent on making his sense-data experiences stand on their own feet, independently of physical objects, that he does not allow them even *psychologically* to point to physical objects. What he strives to establish is a sense-datum language "in which sense-experiences were described by the use of purely qualitative expressions which *carried no reference* to the appearances of physical objects" (125). Ayer admits that sense-data were introduced precisely "by the use of expressions which refer to the perception of physical objects". But this does not hinder us, Ayer argues, from describing these sense-data, once they are introduced, in a neutral way, *qua* sense-data only, without reference to physical objects. In this Ayer may be right. But the crucial question is: If we succeed in so describing them, will they not thereby lose the very property which was the reason why they were introduced in the first place, viz. their ability to function as *evidence* for physical object statements? And will this "description" amount to anything more than mere naming?

If Ayer's concept of sense-datum includes these two requirements, the sceptic's problem as reconstructed by Ayer is insoluble, or rather it is illegitimate since these two requirements are incompatible. For, on the one hand, if we insist on the introductory requirement — that whenever we sincerely make a q-statement, we should be able to bring forward some p-statement as *evidence* for it — how could we do that by bringing forward a p-statement having no reference to the q-statement it was intended to support? On the other hand, if we do insist on this neutrality of p-statements, what reason could there be for invoking them in *support* of q-statements to which they have neither logical nor psychological reference? — However, if the second requirement is reformulated in a less strict way, pointing out that no p-statements must be allowed logically to entail any q-statement, but insisting that — to serve as evidence for q-statements — p-statements must carry a *psychological* reference beyond themselves, Ayer's and, with him, the sceptic's problem would be a genuine one. Such considerations may have induced Ayer to notice that for the formulation of the sceptic's problem it is not really necessary to admit sense-data (124), i.e. sense-data of such a kind as he himself proposes to admit.

Thus, in spite of all our misgivings about the way in which Ayer lets

his sceptic pose the problem of perception, it seems that there is a real problem involved — a problem to be solved, and not merely dissolved. Since the naive realist has been refuted by the very admittance of the problem, there remain only three ways in which solutions may be attempted: the causal theory of perception, the phenomenalist's solution, and Ayer's own solution by descriptive analysis.

The causal theory may be easily discarded, for it turns out that it in no way answers the sceptic's question, viz. "how we are *justified* in accepting physical object statements [q-statements]" (128). What the causal theory proposes is to *explain* the existence of physical objects in terms of atoms and electrons — entities of an unperceived, unobservable kind. However, these entities cannot possibly serve as *epistemological* evidence of our belief in q-statements; they can in no way strengthen our conviction about the existence of physical objects. Something unperceived could never count as a support of our perception of physical objects. "Whatever may be said in defence of the causal theory, it cannot be regarded as furnishing an *analysis* of our perceptual judgements" (128). That is, I take Ayer to mean, it does not give an *epistemological* analysis of q into p and R; it furnishes a mere *mechanical* analysis of an effect into its causes. — I think that not only is Ayer right on this point, but that he is right to such an extent that it is a mistake for him to introduce the causal theory as a method of removing the sceptic's doubt at all. Surely, the scientist does not introduce atoms and electrons in order to justify our belief in the world around us, and any such application of scientific entities seems to be based on a misunderstanding of the purpose for which such entities are introduced.

Phenomenalism, however, has a legitimate claim to be considered as an attempt to answer the sceptic. The phenomenalist tries to close the inference-gap (R) between sense-data (p) and physical objects (q) not, as the naive realist tried to do, by refusing to go below the surface (q), but maintaining that the surface (q) is made of the same stuff as what can be found below (p) or that it is simply the natural limitation of what is to be found beneath it. Empirical statements about physical objects (q) are in reality nothing but sense-data statements (p); physical objects (q) can, by skilful analysis, be seen to be constructions out of sense-data (p); i.e. they are *reducible* to statements, or sets of statements, referring exclusively to sense-data (p) (131). All the difference there is between p and q is that the former kind of statements are on a "lower epistemological level" than the latter kind;

otherwise, in terms of content, there is no difference. The two levels are "logically equivalent" (131—2).

The phenomenalist's position is open to several objections. The first objection that the phenomenalist has to meet is that physical objects cannot be reducible to sense-data since the former, unlike the latter, can exist without being perceived. The phenomenalist meets this objection by explaining that the p-statements into which any q-statement is translatable do not assert that any sense-data are actually occurring, only that "in a given set of circumstances certain sense-data would occur" (133), i.e. that p-statements are predominantly hypothetical. However, this leaves the phenomenalist open to the second objection, that q-statements of a categorical kind cannot possibly be adequately translated into merely hypothetical p-statements. On this point, Ayer defends the phenomenalist's position by claiming that the phenomenalist uses the hypothetical p-sentences precisely to assert that "physical events are occurring", or that "physical objects exist". "This is just what they [i.e. these p-statements] do serve to assert, if phenomenalism is correct," Ayer adds (134). It seems to me that Ayer is grossly begging the question here, for the correctness of phenomenalism is just what is questioned, and the objection considered is precisely that hypothetical p-statements cannot fulfil the function of categorical q-statements.

Whether or not the phenomenalist is vulnerable on this point, it does not matter much, for now comes the third, final and fatal attack on him. It is directed against the phenomenalist's concept of the relation (R) between p and q. This is one of "logical equivalence", the phenomenalist asserts. This can only mean, Ayer says, that a purely deductive relation is supposed to hold between p and q, such that (a) "the existence of a physical object of a certain sort [described in a q-statement] must be a *sufficient condition* for the occurrence, in the appropriate circumstances, of certain sense-data [described in p-statements]" (138), and that (b) "the occurrence of the sense-data [described in p-statements] must be a *sufficient condition* for the existence of the physical object [described in a q-statement]" (138—9). What proves phenomenalism wrong is that neither of these requirements can be satisfied.

To start with specification (b), it requires that the assertion of the existence of a certain physical object (q) should be *logically entailed* by a description of a given series of sense-data (p). To prove this requirement satisfied, the phenomenalist would have to "produce a specimen

set of statements [p], describing the occurrence in particular conditions of certain specified sense-data, from which it *follows logically* that a given physical object exists [q]" (141). Specification (a) requires that statements about physical objects (q) should be formally translatable into statements about sense-data (p) (144). If this requirement ($q \supset p$) were satisfied, the phenomenalist would have to deny the possibility of the existence of physical objects of which no sense-data were obtainable (denying: $q \cdot \bar{p}$), and he would have to assert that from the fact that the requisite sense-data did not occur it would follow logically that the physical object in question did not exist ($\bar{p} \supset \bar{q}$) (142—3).¹³ — On points (a) and (b) alike the phenomenalist fails: statements about physical objects (q) cannot be translated into statements about sense-data (p) in such a way that from the latter (p) can be logically deduced any statement implying the existence of a physical object (q). Ayer might have added that on the introduced conception of sense-datum the phenomenalist is *bound* to fail, for how can a sense-datum statement stand in the relation of logical entailment to a physical object statement if it is to give not logical but *epistemological support* to the latter? The sense-data statements (p) cannot be such that they are proved false by the falsity of a corresponding physical object statement (q), or else they cannot function as *grounds* for statements involving the existence of physical objects.

In the last section of the chapter, entitled "The *justification* of statements about physical objects" (144—8), Ayer is left alone to fight the sceptic by means of the Method of Descriptive Analysis. It is a short fight, and if its outcome appears to be undecided, this is probably because the reporter is Professor Ayer himself. No *justification* of our use of sense-data statements in support of physical object statements is offered, although Ayer does insist that it "*must be possible*" for statements about sense-data (p) to "justify", i.e. *support*, statements about physical objects (q) (148). But there is no real need for Ayer to defend *this* point, for the sceptic does not attack it. It is part of the sceptic's premises that we *do* use p-statements to support q-statements; we *do* pass from p-evidence to q-conclusions. What the sceptic doubts is the *genuineness* of this support, the *legitimacy* of this passage, as Ayer quite clearly has pointed out (76). On this point — and it is at this point that the sceptic attacks — Ayer puts up no defence. When Ayer reaches the "unremarkable conclusion" that the *reason* (a) "why our sense-experiences afford us grounds for believing in the existence of physical objects" is that (b) "sentences which are taken as referring

to physical objects [q] are used in such a way that our having the appropriate experiences [p] counts in favour of their truth" (147—8), this conclusion is therefore open to several objections. First, to explain an epistemological fact (a) by linguistic usage (b), seems at least inadequate, for linguistic usage may be just conventional and arbitrary, which an epistemological fact presumably is not. Secondly, if the epistemological fact (a) to be explained by linguistic usage (b) is supposed to be the sceptic's problem, as presented by Ayer, this is plainly wrong, for it was presented as a problem about the relation between *statements* p and q. Thirdly, and consequently, the conclusion that Ayer offers is a re-posing of the sceptic's problem, for by the explanation offered (b) the sceptic can nicely reformulate his question as follows: Why is it that sentences referring to physical objects (q) are used in such a way that sense-data statements (p) may count in favour of their truth? An answer to this problem does not seem to emerge on the linguistic descriptive analysis level (How could it?), but in the discussion of naive realism and phenomenalism, which are mainly epistemological theories, Ayer makes rewarding excursions into the field of epistemology in a search for the epistemological bases expressed in sense-data statements and serving as grounds for statements about physical objects.

Ayer does, indeed, on one point go beyond mere descriptive analysis of linguistic usage. His point is that in making q-statements on the basis of p-statements "we are elaborating a *theory* with respect to the evidence of our senses." Physical object statements (q) thus "belong to" or function as "part of" a theory, and as such it is but natural that they "*transcend* their evidence [p] in the sense that they are not merely redescriptions of it. The theory [consisting of q-statements] is richer than anything that could be yielded by an attempt to reformulate it at the sensory level [in p-statements]" (147). Quite apart from the fact that this comes very close to granting the sceptic's point, it seems strange to refer to our belief in physical objects as a belief in a theory. At least, we do not *feel* it as a theory; whereas we can seriously discuss the reality of sense-data and the legitimacy of the passage from sense-data to physical objects, the reality of physical objects is hardly ever seriously and sincerely questioned. However, the decisive point against calling q-statements parts of a theory is that they presuppose the *existence* and the *reality* of their objects in a way that theories do not presuppose (but only pose) the reality of their theoretical constructions. The truth and falsehood of a statement about physical objects do not

depend only on the factual descriptive content of that statement, but also on whether or not there is some object, thing or person which the statement is about. What makes us easily overlook this assumption is the fact that statements about physical objects (q) can be *verified* or *falsified* only by means of statements (p) referring to or serving as evidence for the factual descriptive content of the q-statement (cf. 144).

Thus, contrary to what Ayer insists (147), it *does* "greatly matter whether we say that the objects which figure in [q-statements] are theoretical *constructions* or whether, in line with common sense, we prefer to say that they are independently *real*." What Ayer has lost in the course of his analysis of q-statements into p-statements + the rules of transition (R) from p to q, whatever they are, is the *existential element* of q-statements. When the analysis is finished, therefore, Ayer finds that when he fits the pieces of the p-level together again, there is a *logical connection* between the analysans and the analysandum, between "the *way* physical objects appear to us [on the p-level] and the *way* they really are [on the q-level]" (144); "the *information* which we convey by speaking about the physical objects that we perceive [q] is *information* about the *way* that things would seem [p]" (146). But q-statements not only state and describe the *way* things really are; they, in addition, presuppose that things *exist*. By definition (111), this presupposition cannot be expressed in a sense-datum statement (p), and no p-statement or set of p-statements can therefore *logically entail* any statement q, asserting or involving the existence of a physical object. Thus, although logical equivalence is possible between the *way* things appear and the *way* they are, it is not possible "to say of the description of any particular set of appearances that this and only this is what some statement about a physical object *comes to*" (146—7). It is indeed true that a set of p-level statements of actual and possible sense-data may theoretically exhaust the *way* a physical object is, but no p-statement can ever state that the physical object *is*. It is also true that "it is only through their [i.e. the physical objects'] relationship to our sense-experiences that a *meaning* is given to *what we say about them*", and if this were all we *stated* and *presupposed* when making q-statements, and all we *implied* in using them, this observation would be a good ground for saying that physical objects were theoretical constructions. But we state and presuppose something more, viz. their existence, and this is the reason why "the references to them cannot be eliminated in favour of references to sense-data". This

observation ought to have provided Ayer with a good ground for saying that they are *not* constructions, and to conclude, instead of adding as an afterthought, that "they are in any case real in the sense that *statements which affirm or imply their existence* are very frequently true" (147).

These objections are not directed at the method of descriptive analysis used by Ayer, but at his use of it. If Ayer had carried the analysis further, he might quite possibly have found among the elements or premises resulting from his analysis of physical object statements some kind of existential principles, assumptions or premises.¹⁴ He might even have gone beyond that and asked for the conditions under which such assumptions are justly made. But this would have taken him beyond the field of analytics through a study of the epistemological problems of the mind and into the problems of what has traditionally been called "metaphysics" — a name first given to the work of a philosopher who may have travelled precisely along this route.

VI. Statements about the past

Starting from the fact that we do believe in the occurrence of past events and that we do state these beliefs in statements (q) commonly referred to as "statements about the past", the sceptic asks for our *support* of such statements. He asks us to point to that — whatever it is — in virtue of which we feel confident to claim that we know the past. This support or evidence, it is assumed, must be some sort of present *experience*, a "memory-experience", the mere occurrence of which would, under appropriate circumstances, seem to *provide evidence* for the truth of q. This present experience "prompts one", as it were, "to form an accurate belief about one's past experience" (149, 154, 158), and it is therefore thought that if I am able to support a statement about a "memory-belief" (q) by providing a "memory-experience" (p), I have somehow proved my right to be sure of q:

- (q) "Memory-belief" — belief in the occurrence of some past event
 ↑
 (R) Inference or transition from p to q
 ↑
 (p) "Memory-experience" — some content, directly presented

According to Ayer's programme, the sceptic's main attack sets in at R, as an assault upon the legitimacy of the passage from p to q. Further, according to Ayer's plans of defence, there are four main

ways to meet the attack: (1) The Naive Realist maintains that there is really nothing to attack at R, for the simple reason that there is nothing beneath our memory-beliefs (q); past events are directly known, and there is therefore no need or even possibility of asking for any kind of evidence more basic than these beliefs. (2) The Reductionist, on the other hand, wants to entrench himself at point p; statements about the past (q) are all reducible to statements about the evidence (p) that we have or could obtain in favour of them. (3) Taking the Scientific Approach, we may realize the need for defending R directly instead of entrenching ourselves at either q or p, and we would then try to justify the transition from memory-experience to memory as some kind of inductive argument. (4) Finally, the Descriptive Analyst would put up a strange defence, it seems, for he would leave the defence to R itself, confident that the passage between p and q has been made impregnable by constant linguistic usage, and that there is no need to defend it. What we might do instead is to inquire after the reasons why it has withstood all attacks, whereupon we may inform the sceptic about our findings in the hope that he will see the futility of his attack and go home (149–50).

But, obviously, in order to study the relation (R) between p and q, something more must be known about p and q themselves. What kind of memory (q) are we here concerned with? Obviously, we are not here concerned with such cases of remembering in which I merely display some skill, e.g. when I “remember” how to swim or how to count. Nor are we concerned with such cases in which we remember facts merely in the sense that we are able to state them correctly, e.g. that the Battle of Waterloo was fought in 1815. The kind of memory we do seem to be concerned with is what we commonly call “recollection”, the remembering of events. That I *recollect* an event implies that I not only remember *that* a given event (E) occurred, but also that I remember the *occurrence* of E. “Recollection” or “the memory of events” seems to be precisely the kind of memory-belief for which one would feel justified in asking for a corresponding memory-experience which might function as the ground of the credibility of the memory-belief. Consequently, if we are able to state the peculiar characteristic of this kind of memory-belief, Ayer argues, we might quite possibly discover also the memory-experience which is supposed to lie at the basis of it. To that purpose Ayer considers the suggestion that recollection, in the sense of remembering a state of affairs or the occurrence of an event, differs from merely remembering *that* a state of affairs

obtained or an event occurred, in that in recollection the state of affairs or the event remembered is "a fact which one has oneself observed". That is, "our recollection of events is limited to what we have experienced" (160).

However, this suggestion is open to "two fatal objections", of which the more fatal is the following: By being told about some incident in my past I may be fully persuaded *that* the incident occurred, but having this true belief about my own past experience is still not sufficient for saying that I recollect the event. "There is still a distinctive factor lacking" (163), Ayer says. This distinctive factor is obviously what we search for, the memory-experience (p) which turns a true belief in a past event into knowledge or recollection of that event. But what is it exactly that happens when a true belief is transformed into a genuine memory, e.g. when upon being told about an incident in one's past, all of a sudden the incident comes back to one quite clearly? Obviously, what happens may be epistemologically described as a transposal of the grounds of one's belief (q) from the evidence of someone else to some evidence of one's own (p), although Ayer does not consider this way of stating the transformation. He is searching for the distinctive factor which is supposed to be added to the belief in the course of the transformation.

In a special section, "Dispensability of memory images", Ayer at some length considers the claim that the distinctive factor consists in "the presence of a distinctive sort of image" (155). While I think that Ayer's discussion is both penetrating and contributes greatly to the clarification of our concept of a memory-experience, his criticism of the upholders of the image theory seems to me unfounded. Ayer correctly points out that the purpose of the distinctive factor (p) which turns a mere memory-belief, although true, into memory-knowledge must be to serve as "a source of knowledge", not a manifestation of knowledge — it must be "a ground for the acceptance of what is known" or remembered; it must *provide evidence* that my memory-belief is true; it must give me a "*reason to believe*" that the facts were as I remember them (154); it is the peculiar purpose of the memory-experience (p) to give me a right to be sure that q by providing the evidence that q occurred (155).

Having thus determined the function of a memory-experience (p) in relation to the memory-belief (q) of which it forms the epistemological basis, Ayer turns to those who think that p consists in an image, and that, as Ayer construes their (Hume and Russell are especially

mentioned) argument, this image (p) "signifies a past event" in virtue of its fidelity to that event (157). As in the case of Ayer's excursion into the history of philosophy during his discussion of sense-data, what is wrong is his reconstruction of the arguments of his predecessors rather than the construction of his own. The concept of an image in Hume and Russell serves, not primarily to connect a present memory-experience with the present memory-belief of a past event, but to point to the *cause* of such a memory-experience, viz. the past experience of an event. Ayer is engaged in an epistemological, vertical inquiry into the relation between p and q, Hume and Russell use the concept of image to link p horizontally to some event or experience (E) which is psychogenetically prior to p and by means of which the existence of p may be *causally* explained. It seems that concepts like "familiarity" and "fidelity" serve quite well to characterize the relation between E and p.¹⁵ However, Ayer's insistence that a present image (p) can refer to or induce us to a belief in a past occurrence (q) only in so far as it is so interpreted, is important and focuses the attention on the decisive point.

Similar considerations make Ayer conclude, negatively, that "perhaps the correct answer [to our search for the distinctive factor] is that there is *no one thing* that is universally present in every such instance of remembering." Positively, however, he concludes that "there can, indeed, be said to be distinctive *memory-experiences* . . . but these *experiences* do not essentially consist in the presence of a special sort of object." And Ayer probably suggests that the transformation from true belief to genuine memory does not consist in acquiring a state of mind descriptively different from that of true belief, that it consists rather in "giving an assurance that the event occurred, at the same time implying that one is in a position to *know* that it occurred" (164). But to make such a claim, as we have seen from Ayer's general discussion of knowledge, would seem to commit one to giving a good answer to the question How do you know? Merely to answer 'I remember' comes down to little more than a mere repetition of the claim to knowledge because no reasons are given or indicated why one's judgement should be trusted (165—6). We are, therefore, thrown back once more on the question: How can we justify our belief in q by pointing to reasons or evidence that may support it?

At this point, however, the naive realist suggests solving the problem by denying the legitimacy of posing the question; there is no need to justify our belief in memories (q) by pointing to evidence (p) in

its support, since we are acquainted with the past not through some present memory-experience (p) but *directly* in memory-belief (166). Once again, Ayer interprets the naive realist as claiming for memory-beliefs (q) the characteristic that he (Ayer) requires of memory-experiences (p), viz. infallibility, whereas I suppose most naive realists would reject this interpretation and contend that the characteristic they ascribe to memories is nothing but unanalysability, viz. that memories are not inferred from something more basic. Ayer makes his naive realist claim that "some memories at least are self-guaranteeing" (168), that the reason why an act of remembering is thought by him not to need justification is that it is "a cognitive performance which bears on itself the stamp of infallibility" (169), and not that it cannot be further analysed. On this interpretation of his position, the naive realist falls an easy prey to Ayer's objections. For Ayer insists that besides a memory-belief (q) which can be true or false there must be, on a lower level, an experience (p) which as such is neither true nor false, and from which the truth of the corresponding memory-belief (q) does not logically follow (169). Whenever we recollect past events (q), therefore, there *must* be some memory-experience (p), whatever is its content or character, which is then *interpreted* (R) as referring to the past.

We are thus once again thrown back on the *analysis* of statements (q) intended to refer to the past in an attempt to discover the "possible means" we have "of verifying them" (172). But first the reductionist's solution has to be considered. Instead of clinging to the stronghold q, the reductionist claims that there are in reality no q-statements, no statements about the past; he identifies such statements with p-statements: with "statements which are ordinarily taken as referring to the actual or possible evidence on which our beliefs about the past are, or might be, founded" (173). In other words, he reduces our memory-beliefs to actual or possible memory-experiences, to the present or future evidence that is, or might be made, available. To this view Ayer seems to object that it makes the meaning of q-statements "unstable". Since our evidence (p) is continually changing, if q-statements are really p-statements, our statements of past events must be constantly changing, too (173). Ayer's point here seems to strengthen rather than weaken the reductionist's position, for it is a fact that our *belief* in past events does indeed change with our changing evidence.

If, however, the reductionist is represented as maintaining that not

only are our memory-*beliefs* determined by the available evidence, but that the *truth* or *falsehood* of those beliefs is conclusively to be decided by the evidence (174), he can be easily refuted. This is what Ayer does; he makes his reductionist maintain not only that statements about the past are analysable in terms of *meaning* into statements describing what one is actually observing and announcements of our expectations of the future, but that there is, moreover, a *truth-dependence* between these two sorts of statements to the effect that the available evidence formally *entails* a statement that the event occurred (174—5). Such a view of the relation between p and q is inconsistent with Ayer's concept of a memory-experience p as *evidence*, for on this view even though all the evidence (all actual and possible p-statements) goes to show that a certain event *did* occur, we can nevertheless choose to disregard the evidence and claim that the event did *not* occur — without being charged with inconsistency, although we shall no doubt be charged with stubbornness. If the reductionist can be represented as contending that not only are our beliefs made on the basis of our evidence, but that the *past* is manufactured by the evidence, as Ayer makes him say (175), the reductionist position is hardly tenable. I think, however, that it would be fairer to criticize the reductionist for over-looking the truth-relation and paying exclusive attention to the meaning-relation of beliefs and experiences.

In his final analysis of statements about past events, I think that Ayer himself cannot be completely absolved from this charge. For "the *analysis* of a given statement is not affected by the question whether the statement is delivered before, or after, or simultaneously with the event to which it refers" (181), Ayer maintains. In other words, the analysis of a statement depends only on its *meaning*. But, surely, if the analysis is going to serve as a *justification* of my claim to be sure of the truth of my belief in the occurrence of some past event, I must, as Russell points out, "*now* find reason to trust my memory". And this is what Ayer's analysis purports but in important respects fails to do.

Ayer's view of analysis is linked with his concept of statement. A statement, in Ayer's view, is not affected by the time at which or the circumstances in which it is expressed. A statement about a future event, the statement about the actual occurrence of the event, and the statement about its past occurrence are identical or equivalent in terms of *factual*, descriptive content or *meaning*, for they convey the same *information* about the same fact (179—80), Ayer says. Strictly speaking,

therefore, there are no statements as such *about the past, about the present or about the future*. This ought to make one believe that the analysis of statements about the past would correspond exactly to the analysis of statements implying the existence of physical events or objects in the present. The fact that the difficulties of the two analyses are strikingly different, ought to have made Ayer reconsider his concept of analysis. This concept serves well to analyse the meaning of statements into verifiable elements, but this is a *logical* analysis which is bound to yield inadequate results when used for *epistemological* purposes, as we saw in his analysis of physical object statements. His concept of statement is also a logical rather than an epistemological one, for epistemologically (i.e. when the question is not about the *verification* of the statement, but about its *justification*, i.e. when we are asked to determine it as rationally credible rather than true) there is a great difference between statements about the present, the past and the future, and we cannot, as Ayer is well aware, by analysis justify epistemologically these three kinds of statements *in the same way*. These three kinds of statements may all describe the same state of affairs, but *in addition* the first kind assumes or presupposes that this state *obtains or exists now*, the second presupposes that it *obtained*, the third that it *will obtain*. Any epistemological analysis which overlooks these additional elements is to that extent inadequate.¹⁶

In the end, however, Ayer's common sense makes him come out with a more acceptable answer than his concept of analysis would seem to commit him to. Still, the extreme cautiousness, bordering on vagueness, with which this answer is suggested bears traces of his vacillation between standpoints. Some such final answer as the one suggested in his discussion of physical object statements seems to be hinted at as Ayer's final solution. He seems to suggest that statements about the past (q) serve to *explain* our memory-experiences (p) in the same way as a scientific theory explains our observations. This, at least, is the only reasonable meaning I can attach to Ayer's summing up of his discussion: "If the fact that one seems to remember an event [p] is a good reason for believing that it occurred [q], it is *only* because there is *independent evidence* that when someone says that he remembers something the chances are that it was so" (185). The statement that the earth is millions of years old is supported by a wealth of geological evidence; in the same way, Ayer suggests, the evidence for our memory-beliefs "attains a strength which makes it proper to say that some statement about an earlier event is known to be true" (183—4).

It seems to me that this conclusion is open to exactly the same kind of objection as the one I raised to Ayer's analysis of physical object statements; we *do* claim something more for our memory-beliefs and our knowledge of our own past experiences than we claim for our belief in the age of the earth. We are *not* content with claiming it as a mere *explanation* of the occurrence of some present experience; we claim *reality* for it, not simply that it is a convenient theoretical construction which accounts for my memories now. In short, we claim *existential* status for it, although as past only.

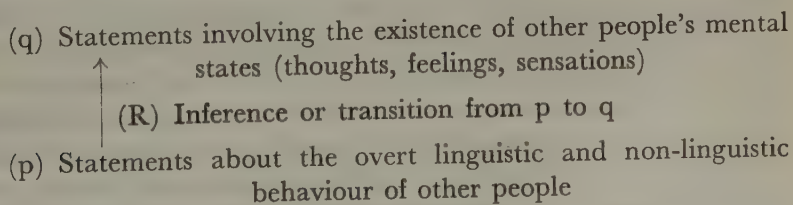
VII. Statements about myself and others

The final chapter of Ayer's book is in many ways the most provocative, but also the least clear. Programmatically, this chapter should treat of the sceptic's problems relating to our right to make the transition from the overt behaviour of other people (p) to their inner thoughts and feelings (q), but the first two thirds of it are devoted to the discussion of related but distinct problems, such as the content of our criteria of identity. Ayer rejects the view that the identity of a person essentially is based on physical characteristics on the ground that we are able to form a concept of personal identity which does not depend for its application upon the identity of one's body (219). In his further search for what constitutes a person, Ayer examines Hume's suggestion that the 'self' is nothing but the bundle or collection of different perceptions (217 ff.). This view, according to Ayer, raises the further question of what is the relation that holds the different members of Hume's bundle together. He finds that some continuity of memory is necessary but that this does not seem to be sufficient, since I do not remember all my experiences. Continuity of memory, Ayer says, "needs to be backed by some other relation of which, perhaps, nothing more illuminating can be said than that it is the relation that holds between experiences when they are constituents of the same consciousness" (225—6).

An examination of Hume's argument for treating the problem of personal identity¹⁷ might have convinced Ayer, as it did Hume himself, that two distinct problems are here involved: (i) how to *justify* rationally our belief in a self, and (ii) how to *explain the origin* of this belief. To the first question Hume found no satisfactory answer, and he concluded therefore, rightly or wrongly, that "all the nice and subtle questions concerning personal identity can never possibly be

decided, and are to be regarded rather as grammatical than as philosophical difficulties.”¹⁸ On the second question — “by what [natural] relations [or associations of ideas] this uninterrupted progress of our thought is *produced*, when we consider the successive existence of a mind or thinking person”¹⁹ — Hume expounds at length, and it is in answer to this second question that he, rightly or wrongly, points to the *faculty* of memory as “the *source*” of the idea of personal identity through the *relation* of cause and effect which it produces: “Had we no memory, we never should have any notion of causation, nor consequently of that chain of causes and effects, which constitutes our self or person.”²⁰ — Ayer may be free to disregard this distinction, but he — and the reader — pay the price for doing so: confusion. Awareness of the distinction, on the other hand, might well have led the discussion away from an examination of the particular *content* of the criteria of personal identity, which may be a matter of emphasis or preference, to an examination of how the criteria of identity *function* in general, why a need arises for the introduction of such criteria and what *purpose* such criteria are felt to serve.

However, the sceptic’s problem about the existence of other minds — to which we turn in the last 23 pages of the chapter — is presented clearly by Ayer as an epistemological problem of the justification of beliefs: “What *justification* can I have for *believing* that there are any experiences other than my own?” (230). It is a question of how to bridge the alleged gap between my observations of the behaviour of other people (p), including their linguistic behaviour, on the one hand, and their inner, mental states (q) on the other. The problem is most succinctly stated as a question of the validity of the inference (R) whereby I, on the basis of statements concerning the overt behaviour of other people (p), conclude that certain thoughts and feelings are present in their mind (q):



Programmatically, there are four methods by which this problem might be solved. The naive realist may claim that the problem does not really exist, since there is no inference from p to q; we are, he holds, directly

acquainted with other people's thoughts, sensations and feelings. The reductionist tries to avoid the problem by maintaining that no one really makes q-statements; we are always on the p-level; if someone talks about other people thinking thoughts of such and such a kind, feeling pain or smelling a rose, what we really mean is that people behave and talk *as if* they thought, felt or sensed in such and such a manner. The man who takes the scientific approach, on the other hand, does not deny that there is a step from p to q, but he maintains that we are justified in taking it by a kind of analogy; by noticing certain connections between our mental states and our behaviour we extend this inductively in application to other people, assuming the same relation to hold in their case. The descriptive analyst, finally, instead of denying the reality of the problem, or trying to establish a logical equivalence between p and q, or attempting to make the stride analogically, breaks down our statements of other people's thoughts and feelings into the evidence on which it is based (p) and the inference (R) which leads us to conclude that there are other minds.

As Ayer presents the position of the naive realist, the possibility of holding it would depend on positive answers to the following two questions: (1) First, can a person *display directly* his thoughts, sensations, and feelings? Can he *exhibit* his experiences without the use of signs acting as intermediary links of communication? (2) Secondly, can I *inspect directly* another person's experiences? Can I directly observe another person's inner state? (231; the two problems are presented by Ayer as one). In other words, the naive realist tries to dispense with level p (statements of outward behaviour), on the ground that we are in direct contact on level q. Ordinarily, his proposal will be met by the objection that we do think that we know a person's feelings (q) from the display of some characteristic behaviour (p) interpreted by a process of inference (R) as a sign of it. But the naive realist may answer, and rightly, that it is at least *conceivable* that we should be in direct contact with another person's inner state. This is shown by the concept of telepathy; whether it is a reality or not does not matter here (231-2). Once again, however, the naive realist's position is made extremely difficult by Ayer's particular interpretation of it. To claim that the p-level is to be dispensed with would mean to Ayer that the certainty and credibility which is usually granted only on the p-level (to our knowledge of our own sense-data and experiences) are claimed on the q-level. The naive realist must therefore be construed as claiming the same kind of credibility for our knowledge of other

minds as we usually grant only to our own sense-data. Now, what Ayer asks of this naive realist is to justify his claim that this is so: "But then it is just the question how such claims are ever to be justified that constitutes our problem" (233). If the naive realist neglects to justify this claim or rejects it as an illegitimate requirement, his "solution of the problem amounts to a dismissal of it. He insists that we do know what we think we know, but he does not *explain* how it is *possible* that we should know it" (233). To Ayer, however, our knowledge of other minds may be easily explained: "Now the *obvious* answer to the question how we know about the experiences of others [q] is that they are communicated to us, either through their natural manifestations in the form of gestures, tears, laughter, play of feature and so forth [non-linguistic p's], or by the use of language [linguistic p's]" (233).

However, it is hard to see how Ayer's attack on the naive realist demolishes his position, even on Ayer's own interpretation of it. The crucial question is surely whether or not Ayer may legitimately ask for a *justification* of the naive realist's claim to know other minds (q) directly. On Ayer's own assumptions this could only be done by pointing to some kind of ground (p) supporting it. Now, a naive realist is someone who denies that our knowledge of other minds (q) is arrived at through epistemologically more basic premises (p), such as the observation of other people's linguistic and non-linguistic behaviour. In objection to Ayer's "obvious" account of how we have such knowledge (q), the naive realist maintains that he knows it directly; there is communication from one mind to some other mind, as in telepathy. His objection is: 'I do not *need* gestures, tears, laughter, play of feature, nor do I need the use of language in order to know what goes on in other minds.' The crux of the matter is that not only does the naive realist not *need* any support (p), but he claims there *is* no such support to be found in the cases to which he appeals. True, our knowledge of physical objects may have to be justified by pointing to sense-data in their support, our claim to know the past may have to be grounded on memory-experiences, but our claim to know other people's inner thoughts and feelings — according to this naive realist — does not depend on any experience or data more basic than that of just knowing what other people think and feel. Ayer requires the naive realist to produce something of which he denies the existence, but such a demand is as unreasonable as to demand of an atheist a proof of God in order to establish what it is he does not believe in.

Ayer next comes to an attempt at solving the problem of our knowledge of other minds that I cannot find any room for among the four possible methods of meeting the sceptic's attack as outlined by Ayer. This may be an indication that his schematism is at least not exhaustive. The view which is here discussed, at one time current in the Vienna Circle, is that we must distinguish between two elements in other people's experiences, viz. the *structure* or the ordering of experiences and the *content* of that experience. What the 'structuralists' maintain is (on the first of the two problems distinguished above, p. 267) that in linguistic behaviour (p-level) is revealed the structure of one's experiences and thoughts (q). This is supposed to provide an answer to the question (the second problem distinguished above) whether I can, on the ground of a person's behaviour (p), validly infer something about his thoughts (q) (234—5). The structuralists found that other people's linguistic behaviour, while revealing nothing of the content of their experience, shows that the structure of their experience is the same as that of one's own (236). — Ayer criticizes the distinction — between structure and content of experience — on the ground that it is impossible to find a "statement which is *purely* about structure" within *descriptive* language (235—6). This is certainly true, but his criticism does not hit the structuralists at all, for their claim is that within any descriptive statement there is a formal, structural relation of a *logical* kind which orders the purely descriptive elements of statements of experience, in a Kantian fashion. This point is explicitly indicated in the title of one of Carnap's books from this period — "*Der logische Aufbau der Welt*" (Berlin 1928).

The structuralist's thesis is not very different from that of the physicalist's, who provides Ayer with the reductionist version on the problem of other minds. However, the thesis of physicalism appears to be a more thorough-going rejection of the possibility of knowing other minds, for not only does it try to show, as did the structuralist, that the *content* of other people's experiences cannot be known, but that it is impossible to know and meaningless to speak about other minds at all; that is, "when one appears to be speaking about [other] minds [q], one is really always speaking about [their] bodies [p]." Moreover, this does not only apply to the minds of others, but also to my own mind, so that, in general, to speak about minds (q) is equivalent to speaking about bodies (p): "To say anything about a person's thoughts or feelings, or sensations, or private experiences of any kind [q], is always equivalent to saying something about his physical con-

dition, or behaviour [p]" (239). The physicalist thus reduces all q-statements to the level of p-statements on the ground that only if q-statements are construed as p-statements, can they convey any information from one person to another.

In illustration of this thesis Ayer turns to an examination of Carnap's distinction of 'protocol language' as "the class of [q-]statements which ostensibly refer to a person's private experiences" from 'physical language' as the "class of [p-]statements which ostensibly refer to physical objects or occurrences" (239). What Ayer attacks is Carnap's view that the relation between these two languages is one of logical entailment or equivalence. As against Carnap, Ayer maintains that, at least "*in one's own case*", there is no relation of necessity between "the sensations, or images, or feelings that one has" and "the physical states or actions by which they are manifested" (241). Now, in the first place, this is clearly not an argument about the justification of the existence of *other* minds at all. In the second place, the thesis of physicalism maintains that — or rather assumes that — one cannot discuss meaningfully the relation between entities so different in kind as sensations and feelings, on the one hand, and physical states and actions, on the other. Moreover, physicalism was not primarily introduced as an answer to the problem of how to know other minds, it was primarily construed as a thesis about the *truth- or meaning-relation* between the statements of physics (i.e. Ayer's "physical object statements" — q-statements in our section V) and other scientific statements, among them, scientific descriptions of the content of one's immediate experience (i.e. Ayer's "sense-data statements" — p-statements in our section V).²¹ The correct representation of the physicalist's argument is thus:

- (q) 'Physical language' — statements referring to physical objects and scientific entities (Carnap, *op.cit.* 44, 81)
- ↑
- (R) translatableness
- ↑
- (p) 'Protocol language' — "statements needing no justification and serving as foundation for all the remaining statements of science" (Carnap, 45)

What Carnap is engaged in showing is that "questions of the *justification* of any scientific statement [q], i.e. of its *origin* in protocol statements [p], involve reference back to the primitive protocol" (Carnap, 44). I do not want to say that the thesis of physicalism is adequate

(the question of *adequacy* can be asked in terms of epistemological *justification*, logical *verification*, or psychogenetic *account of origin* — and the physicalist thesis appears to be brought forward only as an account of the second kind of relation), but its arguments should be properly reconstructed, especially for the purpose of refutation. The physicalist thesis “demands that statements in protocol language [p], e.g. statements of the basic protocol, can be *translated into* physical language [q]” (Carnap, 76). It does so by admitting as genuine p-statements only statements referring to the physical state of one’s body, refusing to talk about sensations *qua* feelings as something over and above what is manifested in overt states, actions, and behaviour (Carnap, 79). This is a serious — and, I think, decisive — limitation, but the physicalist appears to be willing to pay the price for so doing: “We have just considered the *experiences* of various persons and were forced to admit that they belong to *completely separated and mutually disconnected realms*” (Carnap, 80), is Carnap’s conclusion.

Thus, the thesis of physicalism — as expounded by Carnap — is not primarily intended to answer the question that Ayer raises in Chapter 5 — how to justify our knowledge of other people’s thoughts and feelings. It may be so construed, however, if it is given the following form:

- (q) ‘Protocol language’ — statements expressing the experience of others
- ↑
- (R) Translatableness
- ↑
- (p) ‘Physical language’ — statements describing the linguistic and non-linguistic behaviour of others, *as observed by myself*

This is what Ayer does with the physicalist thesis (244–5), but as we have just seen this is precisely what the physicalist objects to: the step from p to q would be a step between two completely separated and mutually disconnected realms. The physicalist’s conclusion is, therefore, that we should keep on the p-level; we may indeed *talk* about the experiences of others, but this talk is then to be so interpreted as to refer only to the overt behaviour of others. Again, this is a severe limitation, but the physicalist is willing to keep within those limits — if q-statements are not interpreted as p-statements, they are “meaningless” because they are non-verifiable.

Ayer’s final answer is construed as a correction of the physicalist’s concept of verification. “The source of the [physicalist’s] difficulty,

here again," says Ayer, "is that one is postulating the existence of something that one could not conceivably observe" (246). Again, his solution consists in distinguishing between "statements which are unverifiable by anyone" and "those that are unverifiable by some particular person." He agrees that I cannot possibly verify another person's private-experiences, but, he maintains, "there is no special class of *statements* about other minds" — "the *meaning* of a statement which refers to a person's experiences is not affected by the fact that it is made by someone other than the person himself" (247) for the "information about the circumstances in which it is expressed is not part of *what it states*" (248). And, since there are generally agreed-upon means of verifying statements, the *justification* of statements about the minds of others consists in an analysis of such statements into *verifiable* premises.

Ayer's answer to the sceptic on the question of other minds is inadequate in precisely the same respect as were his analyses of our beliefs in physical objects and of our recollection of past events. What the sceptic denies is not that statements entailing physical objects, past events and other people's thoughts and feelings can be *verified*. What he asks for is not a proof that the proposition asserted is *true*; he asks me to *justify* my claim that the statement is *not only true*, i.e. that I have not only a *true* belief, but that I have *good grounds now* for being sure of it, i.e. that my belief amounts to *knowledge* in virtue of its credibility. Ayer's distinction between statements "verifiable by anyone" and those "verifiable by some particular person" is highly relevant here, but Ayer chooses the wrong side of the distinction. The question of justification, as against the question of verification, is always directed to *some particular person in some particular circumstance*. This is the reason why, in making the same statement or in asserting the same proposition, *one* person's belief in the truth of what he says may amount to *knowledge* while *another* person's belief amounts to *mere* belief, for the former may have grounds justifying him in claiming the right to be sure, whereas the latter may have no such right since he has no grounds upon which his belief epistemologically rests. In both cases the proposition asserted, the statement made, the certainty with which the belief is entertained, and what would constitute its verification and its truth, may be exactly alike. Still, the former has knowledge, the latter has merely true belief. This difference has to be explained.

The difference between the true believer and the knower of the state-

ment 'This key is made of iron' may be stated as follows: He who *believes* the true statement that the key is made of iron may not know what would constitute the verification of that statement. He who *knows* that the key is made of iron, however, must at least in practice be able to point to some verification, e.g. 'The key is attracted by a magnet'. But the important point is that the knower does not need to verify the statement; for him the statement must already have been verified. Still, he must justify his claim to know, and this he does by pointing to some experience or observation of the same kind as the one used in verification, e.g. 'The key was attracted by a magnet'.

It is easy to confuse verification and justification because any given experience may to one person serve as verification, whereas to some other person, or to the same person at some other time, it may serve as justification. But, again, the experience referred to may not to the same person serve simultaneously as verification and as justification. Successful verification is a precondition of knowledge and must therefore precede knowledge, whereas the question of justification only arises when knowledge is already claimed. Verification seeks to determine the truth of an empirical belief and looks to the future; the justification of an empirical belief tries to discover the grounds of its credibility and for that reason looks to the present and past. Ayer overlooks the important difference between these two dimensions of evaluation of empirical beliefs because he confuses the proposition expressing what is known (which requires verification) with our knowing it (which requires justification).

What the sceptic attacks are the principles, supports or bases which in justification are brought forward as grounds for the credibility of the beliefs for which we claim knowledge status. Whether these grounds are called 'principles' as in the philosophies of Aristotle, Descartes and Leibniz — to mention only a few — 'momentary premises' as in Russell's discussion, or 'terminating judgments' as in C. I. Lewis's analysis, does not greatly matter — but it does greatly matter whether some such bases can be discovered and brought forward to be tested as bulwarks against the sceptic's attack.

- ¹ *The Problem of Knowledge* (London: Macmillan, 1956), x, 258 pp. — Also published in the Pelican Philosophy Series by Penguin Books.
- ² The figures in parentheses are page references to Ayer's book. Here and elsewhere the italics are mine.
- ³ "Proof of an External World," *Proc. Brit. Acad.*, vol. 25 (1939), p. 295: "I can prove now, for instance, that two human hands exist. How? By holding up my two hands, and saying, as I make a certain gesture with the right hand, 'Here is one hand', and adding, as I make a certain gesture with the left, 'and here is another' . . . But did I prove just now that two human hands were then in existence? I do want to insist that I did; that the proof which I gave was a perfectly rigorous one; and that it is perhaps impossible to give a better or more rigorous proof of anything whatever."
- ⁴ *Human Knowledge: Its Scope and Limits* (London, 1948), p. 9.
- ⁵ It should be noticed that the definition is of 'knowing', not of 'knowledge'. This point is significant, for 'knowledge' makes us search for a definition of the kind we usually give of *objects* or things (*per genus et differentiam*), whereas 'knowing' makes us ask what kind of *act* knowing is: What do we *do* when we know (*in* knowing) something? What are the (conventional) requirements for *saying* that we know? And, consequently, what do we presuppose or imply when we claim that we know? Such questions ask for a determination of the context, the circumstances or the situation which set the stage for such an act to be performed. When we claim to know (or grant that someone else knows), we imply to our hearers and presuppose to ourselves that this context and these conditions obtain, i.e. that the stage is properly set.
Many of the important differences between object-definitions and act-definitions may be learnt from contrasting the kind of definition which Aristotle discusses, exemplifies and recommends in his *Analytics*, where theoretical knowledge of things is aimed at, with the kind of definition which he uses and discusses in his *Ethics*, where practical knowledge of actions is the aim. In recent British philosophy the so-called 'Oxford School' (e.g. J. L. Austin and P. F. Strawson) has shown preference for the latter kind of definition. This preference is guided by a change of interest, parallelling Aristotle's switch of aspect, from one function of language to the other — away from the use of language as an organon of science for description to the practical use of language to perform acts (confirming, urging, commanding, recommending, advising, promising, warning, etc.).
- ⁶ My term.
- ⁷ Cf. Aristotle, *Eth. Nic.* vi. 13. 1144b21—22: "All men, when they define [moral] virtue, after naming [ii] the state of mind (habit), and [i] its objects, add [iii] 'that [state] which is in accordance with *right reason*'." Cf. *ibid.* ii.6.1106b36—1107a2. Notice the exact formal correspondence of this definition with Ayer's definition of knowing. — Aristotle in the same way defines the intellectual virtues, among them *knowledge*, although the three clauses are not explicitly stated.
- ⁸ For Ayer's way of connecting his definition of 'knowing' (esp. pp. 13, 33—34) with 'good', cf. P. H. Nowell-Smith's discussion of 'good' in his *Ethics* (London: Penguin Books, 1954), esp. pp. 160—2: "To call something good is, in a way . . . to *vote* for it, to *side* with it, to let others know where I stand. But it does more

than this; it implies that I have *reasons* for casting my vote as I do" (162). "[But it says little] about the reasons. In fact it says nothing about them at all; it only implies that I *have* reasons" (161).

⁹ We shall in this review have to neglect the particular problems arising from the fact that Ayer primarily talks about knowing *statements*, not what the statements tell us or what they are about. Usually we do not claim to know a statement in any other sense than claiming that we recognize or are aware of the statement (cf. 'I know what I say'). What Ayer means here, however, is clearly what is usually expressed by saying: 'I know the fact (state of affairs, situation) stated.'

¹⁰ *Philosophical Investigations* (2. ed.; Oxford 1958), sects. 244, 274, 277–80.

¹¹ *Essay concerning Human Understanding*, Bk. II, Ch. 3, sect. i: "And there is nothing can be plainer to a man, than the clear and distinct perception he has of those simple ideas; which being in itself uncompounded, contains in it nothing but one uniform appearance, or conception in the mind, and is not distinguishable into different ideas."

¹² Cf. *An Inquiry into Meaning and Truth* (London: Allen & Unwin, 1940), p. 124: "We mean by a 'datum' merely a piece of knowledge that is not deduced... The essential characteristic of a datum is that it is not inferred."

¹³ Here, as elsewhere, the formal representation of Ayer's argument is my own, not Ayer's.

¹⁴ Whether the existential presupposition of descriptive physical object statements should be regarded as part of the *analysis* of such statements, may be subject to doubt. If the analyses should contain only what is explicitly *stated*, as against what is implicitly presupposed (and this seems to be most proper since what is analysed is a *statement*), then the formulation of the existential presupposition will be presented not as part of the connotational or denotational 'meaning' of any such statement, but as the specification of the condition, the context or the circumstances in which alone a descriptive physical object statement is properly made. Unless the existence of an object (thing, person) is presupposed, the stage is not set for descriptive physical object statements to be (properly) made or understood. There would then be *no point* in making such statements.

¹⁵ Russell does indeed engage in an epistemological as well as a psychogenetic inquiry, but his use of the concepts of "image" and "familiarity" are distinct in the two inquiries.

Cf. *The Analysis of Mind*, p. 109: "Images, as opposed to sensations, can only be defined by their different *causation*: they are caused by association with a sensation"; p. 80: "When you hear New York spoken of, some image probably comes into your mind, either of the place itself (if you have been there), or of some picture of it (if you have not). The image is *due to* your past experience, as well as to the present stimulus of the words "New York". Similarly, the images you have in dreams are all *dependent upon* your past experience, as well as upon the present stimulus to dreaming."

However, *An Inquiry into Meaning and Truth* is primarily an *epistemological* inquiry. Cf. p. 136: "I must, *now*, find reason [p] to trust my memory [q], and to believe, in suitable circumstances, what comes to me in the form of testimony. I must, that is to say, start from *momentary* epistemological premisses [p]." Russell char-

acterizes this *reason* (p) not as an "image", nor as something "familiar" or "faithful" but as a "yes-feeling"; cf. p. 156: "Images come, it seems to me, in three ways: as merely imaginary, or with a yes-feeling, or with a no-feeling. When they come with a yes-feeling, but do not fit into the present, they are referred to the past." Thus, an experience does not function as reason for a belief in a past event *in virtue of* its reference to the past, as Ayer makes Russell say, but it is referred to the past because of a yes-feeling.

- ¹⁰ The difference between Ayer's logical and the required epistemological analysis may be further clarified by distinguishing them as concerned with, respectively, investigating the *truth* of our beliefs by verification and investigating the *credibility* of our beliefs by justification.

It should be noticed that I am not attacking Ayer's concept of statement. What is wrong, I suggest, is that Ayer analyses *statements* (or other *logical*) entities rather than *beliefs* (or other *epistemological* entities). What needs to be defended against the sceptic's attack is not the truth of statements but the validity of our beliefs expressed in statements. If Ayer had asked for the grounds and justification of *judgments* in the sense of beliefs rather than of *statements* in the sense of propositions, he might have realized that our claim for the validity of empirical knowledge is made for judgments passed and beliefs held at some *particular* moment. Except in the case of *a priori* knowledge, it seems that at each discrete moment the belief which we have of one and the same proposition or state of affairs is different from those held at other moments, and that each of them requires its own justification and ground.

- ¹⁷ *The Treatise of Human Nature*, Bk. I; Part IV; sec. 6.

- ¹⁸ *Ibid.*, ed. Selby-Bigge, p. 262.

- ¹⁹ *Ibid.*, p. 260.

- ²⁰ *Ibid.*, pp. 261–2.

- ²¹ R. Carnap, *The Unity of Science*, tr. M. Black (London: K. Paul, 1934), p. 28: "The following article is an example of the application of Logical Analysis to investigating the *logical relations* between the statements of physics and those of science in general. If its arguments are correct, all statements in science [among them p-statements] can be *translated into* physical language [q]. This thesis (termed 'Physicalism' by Neurath) . . ."

REMARKS ON THE CONCEPT OF KNOWLEDGE

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Suppose we study the language habits of a certain apparently English-speaking tribe, and suppose we find that they use sentences of the type 'I know that ' in such a way as not to make it improper for the speaker to add 'but I may be wrong'. The established use of the expression 'I know' would thus be essentially different from our use of the same expression. The rules for the use of the expression are not the same. What is correct use in the language of the tribe is not correct use in our language.

This is, however, a rather misleading way of describing the difference. Instead we may say one of two things. We may say either that the tribe is using the expression incorrectly. Whatever the people of the tribe believe or agree upon it can never be correct to say that I know that *p* but that I may be wrong. To believe that one may say so is not to have understood the concept of knowledge. Or we may say that although the tribe uses the expression 'I know', it is not at all the same expression as the expression we use when we say 'I know'. Of course, the two expressions are spelt and pronounced in the same way; they constitute one and the same sound- and letter-complex. But although the two expressions have the same physical properties, they are not expressions of the same concept. Despite their use of the expression 'I know' the tribe does not have the concept of knowledge.

The tribe cannot express what it is possible for us to express in our language. We may say that the tribe has not actualized the concept of knowledge; the concept exists only as a possibility. That it exists as a possibility is independent of the fact that there are languages which do express the concept. Even if no language whatsoever used sentences of the type 'I know that *p*' as we in fact use them we should still say that the concept of knowledge exists as a possibility.

It may be regarded as a contingent fact that there are languages in which the concept of knowledge is actualized. It is not a contingent fact, however, but a logical necessity that this concept exists as a possibility in any language in which the concept does not exist actually. But why would it be impossible to imagine a language in which the concept of knowledge did not exist even as a possibility — a language, for example, in which it was logically possible only to issue orders, to warn, and to threaten? To issue an order, to warn, and to threaten presuppose that the person who issues the order, warns, and threatens, knows which state of affairs is ordered, is warned against, and is threatened with. He must, furthermore, assume or believe, but not necessarily know, that something is or is not the case. To order X to be done presupposes the assumption or belief that X is not the case, but it will be if the order is obeyed. To warn against X presupposes the assumption that X is not the case, but that it will be unless certain precautions are taken. And to threaten with X presupposes the assumption that X is not the case, but that it may be brought about by the person who threatens.

It seems, thus, that the concepts of assumption and belief must exist in any language, but that the concept of knowledge does not have to exist. To issue and to understand orders do not require that in addition to the existence of assumption and belief one should be able also to make knowledge-claims.

Wherever the concepts of assumption and belief exist, however, the concept of knowledge must exist as a possibility. Whenever a language provides for the expressions 'I believe' and 'I assume' the expression 'I know' exists potentially.

Whatever kind of language we imagine, it appears to be required that the people using the language pass judgment on states of affairs; such judgments are presupposed in issuing warnings and orders, and asking questions. It follows that it is a deficiency for a language not to apply expressions of the type 'I know' and to apply them as such expressions are applied in our language; it is something which that language does not have, but should and could have.

In any language, therefore, the concept of knowledge must exist as a possibility. Obviously, the concept of knowledge exists as a possibility only inside a language. Neither bees nor dogs have a language and, consequently, they have no concept of knowledge. To have a language is to have concepts; and vice versa, to have a concept is to have a language. Admittedly we do say that the dog knows where it has buried

its bone; we may even say that the earthworm, after so many repetitions, knows its way through the maze. But this use of the verb 'to know' has nothing to do with the use according to which a claim to knowledge is repudiated if sufficient reasons for the claim cannot be stated. To say that the dog knows where it has buried its bone is to say something about what in certain situations the dog will do, and do correctly. The dog cannot state or comment upon its own reactions; it cannot ask questions or raise doubts about its undertaking; it cannot pass judgment of any kind on anything.

The parrot may learn to say 'I know it is Sunday today', but in saying so the parrot is not using language; it is imitating sounds and is not *saying* anything; it is not applying concepts. It does not understand what it means to use the expression 'I know'.

Whatever the genesis of our knowledge of our concept of knowledge may be, such knowledge is not gained by studying the actual behavior of the relevant expressions. If it were so, we should not be entitled to assert, as in fact we are, that we cannot say 'I know that p but I may be mistaken'. All we could say would be that we do not in fact say so; and if in the course of years our speech habits should change to such an extent as would make it customary to say 'I know that p but I may be mistaken' we could not say it was a wrong use of language; we could only say it was a different use. No other criterion for the correct use could be found than that obtained by questionnaires and statistics; and what today is called the logic of language could be pursued only as an empirical investigation.

A possible objection might run as follows. Someone who does not know how to play chess may learn it by observing the actual moves of the chessmen. By observation he may learn the rules according to which the players correctly operate with the different chessmen. In the same way it may be argued that by observing the actual use of expressions of the type 'I know' he may learn the rules according to which these expressions are correctly used — and this is all we mean by having knowledge of the concept of knowledge. It is clearly seen, however, that this objection presupposes that there is a use which is the correct use and, consequently, a use which is a wrong use. It is quite probably the case that the correct use is the actual use; but it is not the correct use because it is the actual use; rather, it is the actual use because it is the correct use; that is, people use the expressions as they do because they know what the correct use is; the correct use is not defined by the actual use. What we can say and what we

cannot say are determined by the concept of knowledge. It is knowledge of the concept that enables us to pass judgment on the right and wrong use of the relevant expressions.

But what does it mean to say that the *concept* of knowledge exists? What are we to understand, in this context, by the word 'concept'? The concept is neither a word nor an expression. The word 'knowledge' is spelt with nine letters, it is pronounced in a certain way and it may be mispronounced; quite often it appears in sentences as a grammatical subject. The concept of knowledge, however, can be neither spelt nor misspelt, neither pronounced nor mispronounced, nor can it appear in sentences. Furthermore, the expressions 'I know' and 'I knew' are two different expressions, and they do not mean the same thing although they employ one and the same concept.

Neither would it be correct to say that the concept is the rule, or rules, for the use of words and expressions such as 'knowledge' and 'I know'. For as we have just seen, the rules may be right or wrong. That is, the rules do not *constitute* the concept; they presuppose it.

The concept is the *rationale* behind the rules. Language is an instrument. It is used in order to do something. It has certain functions. And one of its functions is to make knowledge-claims. In other words, the concept of knowledge is the purpose for which we use such words as 'knowledge' and sentences of the type 'I know that p'. To ask what the concept of knowledge is, is to ask what the purpose is of using these words and sentences. To understand the concept of knowledge is not just to have learned the rules for the use of the relevant expressions; it is to understand *why* they are as they are. It is to understand, for example, why it is incorrect to say that I know that p but I may be wrong, and it is to understand not only that it is wrong but also why it is wrong to deny, *à la* Logical Positivists, the right ever to make any knowledge-claim.

AXIOLOGICAL NIHILISM: SOME THESES

Symposium held at a joint session of the Scandinavian Summer University and the First Scandinavian Congress of Philosophy at Frederiksborg, Denmark, on August 1, 1959.

Reported from a tape recording by Mogens Blegvad. Translated by Bent Nordhjem.*

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Ingemar Hedenius :

Axiological Nihilism is a philosophical theory concerning the properties of certain sentences.

I shall attempt to formulate some theses which I consider reasonable, and which contain only the minimum that is essential to the theory of Axiological Nihilism as originally conceived.

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The sentences to which this theory applies are not formal sentence patterns. Sentence occurrences like 'That is wrong', 'C'est faux' and innumerable others, whether printed or spoken, which share the formal structure of these two, are said to *be* the same sentence: this means that all these sentence occurrences share the same formal pattern. When philosophers theorize about the properties of certain sentences, it is usually these formal patterns they have in mind. But that is not the case when the adherents of Axiological Nihilism discuss the properties of certain sentences.

Every sentence occurrence is said to be a concrete instance of some formal sentence pattern. However, what is meant by the term 'sentence occurrence' is by no means always clear. Sometimes the term obviously refers to classes of concrete instances of some formal sentence pattern. At the top of page 1079 in a book called *Sveriges Rikes Lag* ('The Law of Sweden'), printed in Stockholm in 1949, there is a certain sentence, S. This sentence may be called an occurrence of a formal sentence pattern first published in an Act of April 1, 1807. S is then a sentence occurrence — but it is also a class of 5,000 practically exactly similar sentence occurrences, since *Sveriges Rikes Lag* (89th edition, Stockholm 1949) exists in 5,000 copies. What constitutes the class S and characterizes it exclusively is not the fact that its members have the same formal sentence pattern, but the fact that they share a number of additional properties which do not derive from the common pattern but are connected with the printing, binding, distribution, etc., of a certain book. Sentence occurrences characterized by the maximum degree of concreteness are a different matter, to which I shall revert later.

Axiological Nihilism is not a theory about a number of definable formal sentence patterns and *all* their occurrences. It is concerned only with certain classes of sentence occurrences which share not merely the same formal patterns but also certain other properties connected with the human and social situation which is common to the occurrences in question.

This is the nature of those 'sentences' about which Axiological Nihilism claims to enounce certain speculative truths.

The sentences in question may be termed 'ethical sentences'. It follows from what has been said that ethical sentences cannot be defined in formal terms if by a definition in formal terms we mean a definition of a formal pattern which is common and exclusive to ethical sentences. The best method by which ethical sentences may be

distinguished from other sentences will be to indicate the human situations which are typical of their occurrence.

A corporal tells some soldiers who sleep in the same barracks: 'It is not merely forbidden but also disloyal to one's sleepy fellow-soldiers to talk or make a noise after 9 p.m.' (Example No. 1). A teacher of religion tells the girls in her class: 'Any girl who has sexual intercourse with a man without being married to him is behaving irresponsibly and viciously' (Example No. 2). A politician thinks to himself: 'We have no more important task than that of checking an accelerating inflation' (Example No. 3).

Provided these sentences are used in the given situations in a 'normal' manner — i.e., if they are not quotations (as an example in a grammar is a quotation), nor uttered in fun, nor parts of the reading of a novel or a play — they are ethical sentences. It is irrelevant if (as in Example No. 1) the 'sender' is emotionally insincere, or if (as in Example No. 2) the 'receivers' are entirely indifferent, or if (as in Example No. 3) both sender and receivers by means of the sentence express and fortify an emotional attitude or a readiness to act.

What characterizes those situations which are a necessary condition for the occurrence of ethical sentences is the fact that in these situations the sentences 'normally' express or produce some emotional attitude or some readiness to act. It may be said that the social function of ethical sentences is to express or produce reactions, and that an ethical sentence may occasionally fail, in part or entirely, to accomplish this function. Thus, there can be no ethical sentence unless the situation possesses certain properties that are characteristic of situations where a norm is expressed. One and the same formal pattern may lack the character of an ethical sentence in one situation, and possess this character in another.

To illustrate this point we shall consider two different sentence occurrences which have the same formal sentence pattern, but of which one is an ethical sentence while the other is not. Being in doubt what sentence to pass in a certain case, a judge looks up p. 1079 in his copy of the 89th edition of *Sveriges Rikes Lag* and reads: 'Ej må någon...' (Nobody may...). A sentence occurrence of maximum concreteness has then been realized, and because of the situation this occurrence is an ethical sentence. Next I borrow from the judge the same copy of the same book, and out of idle curiosity (because I want to know how the case will end) I look up the same page and read in my turn: 'Ej må någon...': once more a sentence occurrence of

maximum concreteness has been realized, but although it manifests the same formal sentence pattern as the one realized by the judge it is not an ethical sentence. It is not a norm, but merely a 'quotation' from the law-book. It will be seen that the 5,000 occurrences of the sentence at the top of page 1079 in the 89th edition of *Sverikes Rikes Lag* must be multiplied by a very high number — the number of times the sentence is read by somebody — if we want to consider the sentence occurrences of maximum concreteness. Only some of these occurrences have such normative content that they are ethical sentences and consequently belong to the field of Axiological Nihilism. The question whether Axiological Nihilism is a theory concerning the rules contained in various laws and moralistic works must therefore be answered both affirmatively and negatively. There are genuine ethical sentences; but there are also non-genuine ones, i.e. sentences which merely quote, or state the occurrence of, a genuine ethical sentence; the theory of Axiological Nihilism is concerned with the former kind only, not with the latter.

Certain ethical sentences in English share the formal pattern 'x is right' or 'x is wrong' or 'x is a duty' or 'x is good' or 'x is bad'. Such ethical sentences may be termed *atomic*. What the *x* in these patterns stands for is a debatable point; at any rate, both individuals and classes must be capable of entering into these patterns as subjects. If *x* is a class, 'x is right' expresses a general rule to the effect that an action of this class is right; if *x* is an individual, 'x is right' is a statement about a particular, concrete action.

However, such simple sentences are comparatively rare, both in colloquial speech and in literary English. Generally, ethical sentences have a more complicated form. We may then refer to them as *molecular* ethical sentences. A molecular ethical sentence is a sentence which is not atomic but which logically implies, either alone or combined with some non-ethical sentence, at least one atomic ethical sentence. We assume that all ethical sentences in our language share the formal property of either possessing one of the five atomic structures indicated above, or logically implying a sentence with such a structure. But although common to all ethical sentences in our language this formal property does not exclusively characterize them; many sentences which do not occur in typical norm-giving situations and which consequently are not ethical may display exactly the same formal characteristic.

In what I have said I have attempted to show what phenomena

the theory of Axiological Nihilism is concerned with. In what follows I shall indicate the theses which I find most reasonable. The theses are not the same for atomic and molecular ethical sentences. This is what Axiological Nihilism asserts about atomic ethical sentences:

(1) None of the five predicates which are possible is either a name or a description of a property; consequently, they have no semantic reference.

(2) No atomic ethical sentence is either true or false in the sense in which we suppose scientifically well-formed sentences about spatio-temporal reality to be true or false.

(3) The predicates of atomic ethical sentences are a kind of signs which, although they have no reference, when they are put to practical use indicate (Moore: 'suggest'; Nowell-Smith: 'contextually imply') a certain emotional attitude or readiness to act on the part of the speaker ('sender') and/or the existence of good reasons for the listener ('receiver') to react in the same way. It follows that these signs must be regarded as a kind of assertion signs. If I declare that the earth is round — not as a philosopher quoting an example in a technical discussion, but as a teacher imparting this particular truth to my class — my sentence will contain a kind of assertion sign, which arises from the situation, and which indicates that I, the teacher, am myself convinced that the earth is round and, similarly, that there are good reasons for the listeners, too, to believe that the earth is round. There is no definite symbol in the sentence which has this reference; it is shown by the general context. Analogously, the presence of 'good' in 'x is good' shows us that the sentence may be used to indicate an emotional attitude or a readiness to act.

These were the theses propounded by Axiological Nihilism concerning the semantic, logical, and social properties of atomic ethical sentences. As far as molecular ethical sentences are concerned it is not necessary, according to the theory of Axiological Nihilism to assume an analogue to (1), though one may well assume analogues to both (2) and (3). The indicating of emotive states or motive factors effected by molecular ethical sentences is of course not bound to the occurrence of one or other of the five atomic ethical predicates.

Axiological Nihilism does not deny the possibility that logical consequence may exist between ethical sentences, although it assumes that no ethical sentence can be either true or false.

The above theses have no bearing on the formalization of ethical

language. But they are highly relevant when actual ethical sentences with a material content are subjected to an empirical examination.

It is possible that the theory of Axiological Nihilism may have a wider field of relevance than I have suggested. This would imply that the three nihilistic theses, or the last two of them, apply to more sentences with a normative function than I have suggested. This is an important problem which deserves a full discussion. An equally important subject for discussion would be whether one or more of the theses I have propounded can be shown to be untenable. At least for the present, I am prepared to act in their defence.

Axiological Nihilism may be said to be dubious and difficult to handle. I will conclude by stating some reasons for this opinion.

In the first place, it is, like several other philosophical theories, a speculative hypothesis concerning the properties of certain symbols. The statements which abound in the history of philosophy to the effect that values are not objective — e.g. in Spinoza, Hobbes, and Hume — are connected with the evolution of the natural sciences. They must be regarded as the more or less conscious attempts on the part of philosophers to explain and define their attitude to what has been happening in the natural sciences ever since the Renaissance: the gradual rejection of those value judgements which played such an important part in the Aristotelian view of nature. The need for some philosophical theory of values which would account for the fact that value judgements, or what I have termed ethical sentences, are irrelevant in scientific contexts became even more pressing when a similar reorientation occurred in biology, and finally, at least as an ideal, in the social sciences. In my opinion, the chief argument in favour of the theory of Axiological Nihilism is that it satisfies this need for a theoretical explanation of what the scientists had discovered in their practice: the fact that ethical sentences have no place as integral parts of scientific discourse. What makes Axiological Nihilism a speculative theory is the fact that it is a mere hypothesis, so that one cannot exclude the possibility that the same end might be attained through other theories of value. To find further support for the theory of Axiological Nihilism, it would therefore be necessary to cite reasons, primarily of a philosophical nature, for believing that it is superior to any possible rivals. But it would be impossible to suggest any empirical line of inquiry which might make the acceptance of Axiological Nihilism obligatory.

Next, the question of its field of applicability is a complex and

insufficiently clarified problem, which will now be taken up by Per Ericson. Further developments in this sphere may necessitate a modification of the definition of ethical sentences which I have suggested.

Per Ericson :

According to Ingemar Hedenius, the theory of Axiological Nihilism applies only to affirmative atomic ethical sentences (*a*-sentences). This means that the theory cannot help us to interpret sentences composed of several *a*-sentences (this group includes sentences like (*a*) 'Ought $p \supset$ ought q ' and (*b*) ' $p \supset$ ought q ', but not a sentence like (*c*) 'Ought ($p \supset q$)'; (*c*) is often confused with (*a*) and (*b*)), negations of *a*-sentences, and modalized *a*-sentences. It also means that the theory is not concerned with existential or universal propositions like 'There exist just wars' and 'All dictators are scoundrels'. We shall apply the term '*m*-sentences' to all ethical sentences which are outside the scope of Axiological Nihilism, thus conceived.

Such severe limitations in the applicability of the theory are obviously undesirable from a scientific point of view. We should therefore ask ourselves the question whether the evident similarities between *a*- and *m*-sentences are not more relevant to the theory concerning the interpretation of ethical sentences than any dissimilarities which, according to Axiological Nihilism, obtain between them. The non-nihilistic theories do not limit their own field of applicability in this manner.

Would it be possible to extend the field of Axiological Nihilism so that it would cover *m*-sentences, too? That is the question that inevitably arises. The following line of reasoning seems natural: According to Axiological Nihilism, an *a*-sentence like '*A* is good' is not a sentence about reality: the ethical term in the sentence has no semantic reference. But in given circumstances the sentence indicates that the speaker approves of *A*. One might then consider interpreting the negation 'Not: *A* is good' analogously: as indicating that the speaker does not approve of *A*. An implication including two *a*-sentences — e. g. '*A* is good \supset *B* is bad' — might similarly be interpreted as indicating that if *A* is approved of, then *B* is disapproved of. And a modal sentence like 'It is possible that *A* is good' might be interpreted as indicating that *A* may possibly be approved of. It would be more difficult to apply the same principle of interpretation to sentences like 'All criminals are maladjusted'.

What grounds exist for rejecting these interpretations and adhering to the view that Axiological Nihilism applies to *a*-sentences only? Let us begin by considering negations of *a*-sentences, which may be said to be basic to our problem, and discuss some arguments that may be adduced for excluding *m*-sentences of this type from the field of Axiological Nihilism:

(1) It is generally assumed that there is some sort of isomorphism between linguistic sentences on the one hand and that which the sentences are said to express on the other hand. Let us suppose that what corresponds to the sentence '*A* is good' is an emotional attitude of approval, and that what corresponds to the sentence '*A* is bad' is an emotional attitude of disapproval. Thus, the incompatibility of the two emotional attitudes corresponds to the contrary relation obtaining between the two sentences. Now, on the principle of isomorphism, the emotional attitude corresponding to the sentence '*Not: A* is good' ought to be not merely incompatible with but also contradictory to the emotional attitude expressed by '*A* is good'; however, it seems absurd to attribute such characteristics to emotional attitudes. It may, perhaps, be suggested that the sentences in question express emotional attitudes only in the sense that they indicate that a certain attitude exists. If so, '*Not: A* is good' might be interpreted as indicating that *A* is not approved of, and the principle of isomorphism would be satisfied. But against this interpretation it may be argued that the sentence '*A* is good' would not indicate the existence of any emotional attitude if it did not express approval in the sense in which we first used the word.

(2) It may also be objected that '*Not: A* is good' lacks the emotional content that is present in the corresponding positive sentence. The sentence '*Not: A* is a nasty scoundrel' does not sound very emotional. Axiological Nihilists often criticize the naturalistic theories of value for failing to do justice to the emotional content of value judgements. Perhaps, this objection does not apply to negations of *a*-sentences and other *m*-sentences.

(3) A similar difficulty arises if value judgements are interpreted imperatively. If a positive statement is interpreted as recommending a certain course of action or a certain object, it is difficult to apply the same principle of interpretation to the negation of a value judgement, which seems to lack any imperative content. It would evidently be incorrect to interpret the negation of a normative judgement, say an 'ought' sentence, as recommending the omission of a certain course

of action; this would lead to the elimination of the distinction between 'ought -p' and 'ought p', and similarly between 'good -p' and '-good p.' But the fact remains that such sentences are often used synonymously.

(4) As another argument in favour of the view that *m*-sentences are not emotional sentences (in the sense which Axiological Nihilists ascribed to this term), it may be pointed out that a person who makes a purely theoretical statement cannot reasonably be said to be indicating an emotional attitude. But the assumption that an ethical sentence can never be logically implied by a theoretical one is highly dubious, and incompatible with the existence of so-called mixed inferences, i.e. inferences whose premisses include both theoretical and ethical sentences. If *t* is a theoretical sentence, and if v_1 and v_2 are value sentences, and if *t* and v_1 logically imply v_2 , it will also be true that the theoretical sentence *t* logically implies the ethical sentence $v_1 \supset v_2$. All the same, it does seem odd to regard the consequence in the following implication as the expression of an emotional attitude:

'There is nobody who is the president of Sweden' implies 'Not: the president of Sweden is a scoundrel'.

These were some of the arguments against including certain types of *m*-sentences in the field of applicability of Axiological Nihilism; and similar arguments could be cited against many of the other types. What characterizes all these sentences seems to be that, in contrast to *a*-sentences, they do not evaluate anything: they do not state that something is good, bad, etc.

However, if the above arguments against applying the theory of Axiological Nihilism to *m*-sentences are tenable, they may also be adduced as arguments against applying the theory to *a*-sentences, with the possible exception of sentences of the type 'This is good'. For, if the theory of Axiological Nihilism is found to be inapplicable to, say, the negation of an *a*-sentence, would it not be natural to examine whether whatever theory is applicable to *m*-sentences might not also be applicable to *a*-sentences? At any rate, it is reasonable to assume that *a*-sentences may be interpreted in a manner analogous to that in which *m*-sentences are interpreted. It is conceivable that the theory of Axiological Nihilism is only concerned with a special manner of using an extremely narrow class of ethical sentences, and that this theory is quite compatible with other theories of value which assume that ethical terms have some sort of semantic reference.

However, two other possibilities, both of them consistent with the theory of Axiological Nihilism, should be considered.

The first possibility is that Axiological Nihilism applies to both *a*- and *m*-sentences, but that that portion of the theory which is concerned with emotional attitudes is relevant to *a*-sentences only: *m*-sentences might then be said, *either* to evince a higher degree of meaningfulness than *a*-sentences in so far as they are not merely theoretically meaningless but do not even perform the psychological function performed by *a*-sentences, *or else* to have another psychological function than *a*-sentences. In the former case, *m*-sentences would acquire a status similar to that of, say, the negation of an imperative.

The other possibility, which is also consistent with Axiological Nihilism, is that *m*-sentences might be regarded as so-called spurious or external value judgements. According to this interpretation, an *m*-sentence would be a statement about the existence of certain valuations — it would not express them. But it seems unlikely that this interpretation would be applicable to all *m*-sentences; for that would mean that all *m*-sentences were of the elliptic character typical of spurious value judgements.

Harald Ofstad:

The fact that the two first speakers paid so much attention to the field of applicability of Axiological Nihilism is typical of the situation of the theory of value today. From being a set of rather catchy postulates asserting that norms and valuations are mere emotional expressions incapable of being either true or false, the theory of Axiological Nihilism is now being confronted by all the formidable problems connected with the delimitation of the phenomena about which a theory of value is meant to be a theory. First, it is clear that norms and valuations may, in principle, be expressed in non-linguistic behaviour; it is therefore important to bear in mind that, simply by concentrating on linguistic expressions, philosophers have considerably narrowed the field of inquiry. Secondly, as soon as we begin to look for criteria by which linguistic expressions of valuations and norms may be recognized, we are faced by the difficulty that the grammatical form cannot in itself be decisive, since sentences containing typically ethical words like 'good', 'right', 'bad', etc., may be used in certain situations in a purely descriptive way. On the other hand, a sentence which from a grammatical point of view would appear to be purely descriptive

or factual — e.g. a sentence like ‘This is contrary to human nature’ — may be used in order to express a norm or a valuation. This fact makes it necessary to speak of a special *normative or evaluative use* of language.

Even supposing we were to discover criteria by means of which we could recognize the normative or evaluative use of language, we should probably be faced by a class of phenomena too comprehensive to be covered by any one theory. We might have to formulate different theories for different types of norms and values, one for moral ones, one for legal ones, one for aesthetic ones, etc. These are important and difficult problems, on which, however, I shall not dwell in this context.

Let us imagine that we have succeeded in delimiting a certain class of phenomena which we may call the *moral use* of language. It would still, I think, be impossible to invent a theory of value to cover this class, even if we narrowed our scope to a limited class of language users, say, Scandinavian philosophers. The only theory I could imagine would be a pluralistic one which assumed that the expressions in question were used in a great variety of ways.

In certain situations a moral sentence may be susceptible of a purely cognitive interpretation. In other situations such a sentence may have imperative force or, perhaps, the function of a recommendation. There are also situations in which none of these interpretations suffice — for instance if, speaking to oneself, one says, ‘I ought to do this or that’, simply expressing a commitment, without any social function. Any attempt to generalize one specific interpretation is doomed to failure. Consider the theory that all moral use of language is recommendatory. It works in some cases: A moral judge who is in doubt whom he should use for a certain job wonders ‘Shall I ask A or B?’. If my answer is, ‘A is a good man’, this is a kind of recommendation. But there are obviously moral dilemmas in which there is no question of anybody wanting to ‘sell’ any particular alternative. Similarly, although emotions are often involved when moral sentences are used, it is not specific to them, since that may be the case also when language is used for scientific purposes: plenty of emotion may be expressed in a discussion between adherents of conflicting scientific theories. The emotional element varies with other factors. What matters is not whether language is used evaluatively or for scientific purposes, but whether the speaker feels on sure ground or not. A confident speaker will often be emotionally neutral. The main task

facing us is to describe the circumstances under which moral sentences have one or the other function. Therefore I cannot accept the first of the theses proposed by Professor Hedenius. Even in the situations in which 'good', 'right', etc. are used with an imperative or recommendatory function, these words refer to something. Even if it may be proved that they express or indicate emotions, that is no proof that they cannot also perform another function.

I am also critical of the formulation of the second thesis: that moral sentences are incapable of being true or false, but I cannot go into this question here. However, the problem whether norms or value statements can be true or false is really not the crucial one. If we approach the problem subjectively and ask ourselves in what sense it is important that norms and valuations can be valid, we shall soon discover that we do not often want them to be true or false in the sense in which scientific statements concerning space and time are true or false. There are important aspects of life about which we should not like it to be possible to formulate true or false norms. If every precisely formulated normative or axiological sentence were comparable to a scientific one, such sentences as 'You ought to wear a tie' or 'You ought to have your shoes polished' would either be true or false, with the result that life would become intolerable. What is important is that certain basic moral principles should be valid, so that a sentence like 'It was wrong of Hitler to gas the Jews' can be shown to be valid, or at least more valid than the opposite statement. If we want to achieve both results — to make it possible for some, but impossible for other norms and valuations to be valid or invalid in a certain sense — it is obvious that what matters is not truth or falsity in the sense in which these terms apply to factual statements.

Instead, we should try to discover other criteria of validity. As a tentative solution I would like to suggest that we might adhere to that tradition in philosophy (represented among others by the Stoics and Spinoza) which accepts as valid those norms which agree with human nature. This rule is vaguely worded; but it might serve as a point of departure for a search for a more precise wording which might be accepted as interesting and useful, though not necessarily the only correct one. The following is one such suggestion with special reference to systems of norms:

The system of norms N_1 is less valid (more invalid) than the system of norms N_2 if, and only if, the following conditions are satisfied:

(a) There exists a set of psychological phenomena, P , which are more closely correlated with the observance of N_1 than with the observance of N_2 .

(b) Those who obey N_1 or N_2 are powerless to decide whether P shall occur or not.

(c) They are powerless to adopt any other attitude to P than one of negative valuation.

An extreme example: Suppose I am arguing with a fanatic anti-Semite. If I can prove to him that he and other followers of his anti-Semitic system of norms are more liable to get peptic ulcers than followers of a more tolerant system, then I have shown, according to the above criterion, that his system is less valid than the other system.

It seems to me more important to try to discover new criteria of the validity of basic moral norms and valuations, than merely to continue with the discussion of their truth or falsity.

Ingemar Hedenius :

Per Ericson made a number of interesting points which, from the point of view of Axiological Nihilism, appear paradoxical. He said that if his observations are true (and I think they are substantially true), the implication is that the theses I proposed must be either rejected or modified. I can think of no smaller modification than the one Per Ericson suggested, i.e. the differentiation between 'ethical sentences' in the original sense, corresponding to my definition, and 'ethical sentence' in a wider sense, which would also include sentences that lack certain properties found in ethical sentences in the original sense, viz. the emotional colouring and the social function of indicating an attitude or a readiness to act. I think one might regard an ethical sentence in the secondary sense of the word as a kind of necessary middle term in ethical argument and as being, like ethical sentences in the primary sense of the word, incapable of truth or falsehood.

To Professor Ofstad I want to say that his contribution was essentially a profession of faith. He said that he thought an acceptable theory of values would have to be pluralistic, that he thought there was no difference in principle between the verification of sentences reporting an observation and the verification of ethical sentences, etc. It is

difficult to discuss such statements in the absence of any indication of what is wrong in the theses of Axiological Nihilism and why they do not possess the general validity they claim to have. According to the semantic notions of Axiological Nihilism, Professor Ofstad's belief that words practically always have a referential function, apart from any other function they may have, is a mere delusion. When speaking of 'justice' one generally has a feeling that the word refers to a certain sum of qualities. On closer inspection this feeling is seen to be a delusion due to the fact that the use of the word is restricted to a certain class of objects, e.g. actions, distributions, and conditions. It is impossible to suggest any definite characteristics which are shared by all objects that are called 'just'; but the word may be said to be used correctly so long as it is used within this sphere of objects. It does not follow that it has any semantic reference.

Harald Ofstad :

When I maintained that words like 'good', 'right', etc., are rarely — I may even say never — used without any semantic reference, I certainly did not mean that they could only refer to a limited sphere of objects. This is what I meant: When I use the word 'right' about some action in a typical normative situation, I refer, e.g., to certain expectations which I entertain concerning the consequences of this action; thus, it would be misleading to say that the word 'right' had no semantic reference. If I call a man 'good', there are a great many factual qualities he must have to be called 'good', and to which I refer by means of the word 'good'. On the next occasion on which I maintain that a man is good, I shall not *necessarily* be referring to the same complex of qualities; but whenever I use the word there is such a complex. Is that not Ingemar Hedenius's view?

Ingemar Hedenius :

May I begin by referring to my distinction between atomic and molecular ethical sentences? As far as I can see, the theory of Axiological Nihilism need not deny that predicates in molecular ethical sentences can have semantic reference, and some of the cases that Harald Ofstad is thinking of are presumably molecular sentences.

The delusion I mentioned has another facet which I want to point out. As a result of the general situation a term with no reference may point in a certain direction. If we know that all persons present in a room are adherents of a graduated system of taxation, and if one

of them declares that the system of taxation in a certain country is just, we may draw the conclusion that the country in question has some sort of graduated system. It does not follow that the word 'just' should be interpreted as meaning or implying 'graduated'. The implication is much vaguer and corresponds to what Nowell-Smith calls 'contextual implication', which does not depend on any semantic reference in the predicate.

Harald Ofstad:

It seems that Ingemar Hedenius and I are using the words 'semantic reference' differently. Perhaps we had better avoid this ambiguous term and try to restate the problem. I suppose it is an essential characteristic of a motor-car that it should possess at least three wheels. If I declare that a certain object is a motor-car, one may question whether I am referring to the fact that it has at least three wheels. But if I am informed that it hasn't got three wheels but only two and a half, I shall be ready to *retract* my assertion that it is a motor-car. Could we not employ this phrase and ask if we should not be prepared to retract our norms and valuations if we were confronted by certain items of empirical information? Similarly, if I have stated that a certain man is good but have subsequently been informed that he is in the habit of beating up his wife twice a day, I shall certainly retract my valuation.

Finally I would ask Ingemar Hedenius to comment on my thesis that it is not interesting whether ethical sentences can or cannot be true or false.

To this *Ingemar Hedenius* replied that he did not share this view. Some people might consider it important to ascertain the truth or falsehood of norms. But in any case it was difficult to decide what was interesting and what was not; that would always be a matter of taste.

In the general discussion *Carl Jørgensen* criticized the symposiasts for having failed to distinguish between norms and valuations. A norm is prescriptive, imperative, or recommendatory; a value is a kind of quality. Replying to this, *Harald Ofstad* said that by using the expression 'values and norms' he had shown that he realized the importance of the distinction.

Sven Krohn maintained that there are genuine value judgements which are true or false. He agreed with *Harald Ofstad* that it would be intolerable if all 'ought' sentences had to be either true or false.

Therefore it was necessary to set up a third value: 'indifferent'. Commenting on Ingemar Hedenius's analysis of the reasons for our interest in Axiological Nihilism as a theory of values, he said that a theory of that kind was only called for if the results of the natural sciences were accepted as a kind of materialistic metaphysics, not if they were regarded as merely one manner of explaining a portion of our experience.

Torstein Eckhoff thought that Axiological Nihilism originated in a feeling that there is a fundamental difference between, on one hand, ethical sentences and possibly certain other sentences of a similar kind and, on the other, theoretical assertions. However, he continued, it is doubtful if the theses which have been proposed are capable of serving as criteria for this distinction. The fundamental difference may derive from our attributing different structures to reality in valuation and in description. If subjected to a scientific description, human behaviour will be structured as spatio-temporal processes in which relations of causality or probability are essential; but if human behaviour is subjected to a valuation, the emphasis will be on other relations, among which duties and promises, justice, etc., are essential. One of the reasons why we tend to assert that only descriptive sentences can be true or false may be that they are somehow isomorphous with the sentences formulated in the natural sciences, which have a high prestige, whereas there is no science that structures reality as we do in our everyday valuations.

Harald Ofstad replied that he had no objection to what *Torstein Eckhoff* had said if it was to be understood as a psychological description. *Ingemar Hedenius* regarded *Torstein Eckhoff's* contribution as an attempt to formulate a rival theory of values. If he had understood it correctly, it implied that it would be possible, by applying scientific methods to ethical relations, to construct a science — parallel with, but different from, the natural and social sciences — within which ethical sentences could be proved or disproved. This did not seem very acceptable, and at any rate he doubted if the theory would be applicable to other valuations than specifically ethical ones.

Knut Erik Tranøy asked *Harald Ofstad* how his proposed criterion of validity was affected by cultural relativity. It is conceivable that peptic ulcers are produced, in some cultures, by anti-Semitism but, in other cultures, by tolerance. If so, comparisons between two systems of norms are only possible within one and the same culture, and the question arises how narrowly a culture should be defined. Another

problem is to define the difference between the pain inflicted on a man against his will, and a real punishment.

Harald Ofstad replied that if the distribution of peptic ulcers could be proved to be as imagined by Knut Erik Tranöy, he would regard this criterion as useless and attempt to find another one which was independent of cultural differences.

Stig Kanger had wondered how Ingemar Hedenius would analyse the sentence 'Everything that I approve of is bad'. The sentence might be uttered in an access of moral lucidity and be a genuine ethical sentence according to Hedenius's criteria. If so, it would seem to contain a paradox.

Harald Ofstad maintained that there was no self-contradiction in the statement that a person approves of something of which he disapproves: he might be a split personality. Ingemar Hedenius pointed to the words 'and/or' in his third thesis, which indicate that the use of an ethical sentence does not necessarily indicate an attitude of approval, etc. The simplest analysis of the sentence 'Everything that I disapprove of is good' is that the speaker indicates that there are good reasons for the listeners to approve of that of which the speaker disapproves.

Konrad Marc-Wogau supported Ingemar Hedenius's conception of semantic reference and disagreed with Harald Ofstad. Naturally, 'semantic reference' may be variously defined, but there is an important distinction which should not be blurred. When I say that the word 'red' refers semantically to beams of a certain wave-length, that is true of all red objects; but if I say that a man is good, the qualities I may be said to refer to in the man are not the same whenever the word 'good' is used. There is no doubt that this distinction is essential to Axiological Nihilism and that it may be expedient to restrict the term 'semantic reference' to the former type of word.

Harald Ofstad thought that the difference could best be expressed by a distinction between words which have an unstable semantic reference and words which have a *stable* reference. Moreover, he doubted if the word 'red' was representative of non-ethical expressions. The semantic reference of many non-ethical expressions was probably much less stable than that of some ethical terms.

Ernst Percy said that if it is asserted about a person that he is a scoundrel this sentence refers semantically to the behaviour displayed by the person in question; it follows that a sentence of that kind is a statement of facts and not a valuation.

REPLY TO MR. STIGEN

by

A. J. Ayer

Oxford University

Mr. Stigen's criticisms of my *Problem of Knowledge* are very searching, and I doubt if I have a satisfactory answer to them all, but I will do the best that I can. My method will be to consider his main objections in the order in which he raises them.

I. On the Definition of Knowledge

Mr. Stigen does not seem to dispute my view that the conditions for knowing that something is the case are first that the proposition in question be true, secondly that one be sure of its truth, and thirdly that one should have the right to be sure, though for a reason which is not clear to me he doubts whether this should be called a definition of knowledge. He does, however, complain that I do not make it sufficiently clear what I would regard as affording one the right to be sure. Because I draw an analogy between the definition of knowledge and the definition of good, he fears that my readers may be misled into thinking that I take "being sure" to be dependent on our will, in the way that moral actions are, and so make the bestowal of the right to be sure a question merely of choice and decision. But surely I must allow that it is in virtue of some rational, intellectual, quality that knowledge is to be distinguished from mere true belief.

I agree with Mr. Stigen here at least to the extent of not wishing to imply that in claiming that a belief amounts to knowledge one is attributing a moral quality to it. The analogy which I draw between the definition of knowledge and the definition of good was intended only to bring out the point that in defining an evaluative term one is not bound to specify the standards on which the evaluation is based: I did not mean to suggest that the standards themselves were of the same kind in each case. On the other hand I do think that the question

when to claim or concede the right to be sure is in a certain sense a matter for decision. I hope to make clear what I mean by this.

The position would be simpler if it were always requisite that the claim to knowledge should be backed by evidence. There are indeed those who think that this is so, and some of them have suggested that what qualifies a true belief as knowledge is just the strength of the evidence on which it is held. In fact they would actually define knowing as believing a true proposition on sufficient evidence. But then they are faced with the question whether this evidence must itself be known. It would seem that it must, for otherwise it would not serve its purpose. If I am asked how I know that a given proposition p is true, it is not enough for me to cite some other proposition q which strongly supports p , even though it happens that q itself is true. If I do not also know that q is true, I may have given a reason for the truth of p , but I have not given a reason for claiming that I know it. But if in every case in which I claim to know a proposition I am stating that I have sufficient evidence for it, then I cannot know q unless I know some further proposition r , and cannot know r unless I know some further proposition s , and so we are led into an infinite regress. This infinite regress need not indeed be vicious. It is clear that if I try to set forth my reason for claiming to know that some proposition is true, the process must stop somewhere: sooner or later I must succeed in adducing a proposition my knowledge of which is allowed to go unchallenged. In every such case, however, it is possible that if I were challenged I could justify my claim. It will then be a matter for decision where I choose, or am permitted, to bring the process to an end.

An alternative, and I think a sounder, view is that there are some propositions that we can claim to know without having to know any other propositions as reasons for them. The most obvious examples are propositions about one's own present thoughts, or sensations or feelings. If someone claimed to know that he was in pain or that he was day-dreaming about being rich, it would seem absurd to ask him what evidence he had for believing that these things were so. What could he answer beyond insisting that he really did feel pain or that he really was imagining himself to be rich? In such cases the question "How do you know?" is out of place. For this very reason, indeed, Wittgenstein and his followers have denied that these are cases of knowledge, but here they seem to me to be placing an unjustified restriction on our use of the verb "to know". It is true that we

very seldom have occasion to employ such phrases as "I know that I am in pain", but the reason for this is that the information which they convey is gratuitous, not that they convey no information at all. In the normal way, if someone is in pain, we take it for granted that he knows it.

But even in cases where it is pertinent to ask "How do you know?", I am not sure that this question must have an answer for the claim to knowledge to be justified. As I remark in my book (p. 32), if someone were repeatedly successful in making true statements in a given domain, "we might very well come to say that he knew the facts in question, even though we could not explain how he knew them. We should grant him the right to be sure simply on the basis of his success." If the achievements of crystal-gazers or clairvoyants or people with a gift for telepathy were a great deal more impressive than they are, they might provide examples. I am not saying that even if a person of this kind were invariably successful we should be bound to credit him with knowledge. So long as he could produce no rational ground for his statements, we might refuse to allow that he really knew that they were true. We might suppose that he had been having a lucky run, which could not reasonably be expected to continue. But if we were persuaded that his successes would continue, then I think that it would not be reasonable for us to reject his claim to knowledge. For what is the point of adducing grounds except to give us this assurance? If we confidently accept p on the evidence of q , it is because we have learned from past experience that a fact of the type that is stated by p is likely to be conjoined with a fact of the type that is stated by q . Even if the connection between p and q is deduced from a highly abstract theory, the theory itself is accepted only because such deductions from it have proved to be reliable. In the last resort the right to be sure is always grounded on some record of past success.

Now we can lay down rules which prescribe what manner and degree of past success is to count as legitimate backing for a claim to knowledge; and we can on this basis define "the rational quality" of knowledge "that sets it above mere true belief". But this does not mean that we can rule out the element of decision. For if these procedures themselves are challenged we cannot prove that they are rational; or rather, we can prove it only if we beg the question. One of the main points which I have tried to bring out in my book is that the procedure of the sceptic in philosophy is always to raise the

question whether what we take to be good evidence, in one domain or another, really is so; and one of the main results of my enquiry is that on its own terms the sceptic's challenge cannot be met. We cannot justify induction, without at some point assuming its validity; we can vindicate one statement about the past by appealing to the truth of another, but we cannot give a general proof of the legitimacy of inferring from present records to past events. The result is that we defend ourselves against the sceptic simply by denying him the fruits of his victory. We postulate that certain inductive procedures afford us a reliable guide to the future: we postulate that certain present happenings are reliable indications of the past. There may well be others, besides Mr. Stigen, to whom this conclusion seems unsatisfactory, but I think it is demonstrable that it cannot be avoided. We might indeed take a different decision; we are not, for example, logically bound to adopt the inductive policies that we do; but the necessity of taking some decision is unavoidable.

II. Perception

Mr. Stigen demurs to my saying about one of my examples of a sense-datum statement that "we must take it to refer only to what is 'visually given' to me, irrespective of its connection with anything else." I look upon sense-datum statements as providing the evidence on which all our characterizations of physical objects are ultimately based, and he does not see how it is possible that they should do this, if they are construed in such a manner that they in no way "go beyond" mere sensations. But here I think that Mr. Stigen may have misunderstood me. When I spoke of the sense-datum statement as referring to what was visually given "irrespective of its connection with anything else", I meant only to say that it was not to be understood as logically entailing the existence of anything beyond the content of the sensation; and, in particular, I took this to imply that it did not entail any statement which asserted the existence of some physical object. In other words, the sense-datum statement could be true even though, as in the case of an hallucination, the perceptual judgment to which it gave rise was false. But this does not mean that I was necessarily fixing a gulf between the respective classes of statements about physical objects and statements about sense-data. Even if it followed from my way of introducing sense-data that no single sense-datum statement could on its own entail a statement which

implied the existence of some physical object, it did not follow that the same would apply to any collection of sense-datum statements, taken as a whole. It is true that in the end I found reasons for rejecting the phenomenalist view that physical objects are "logical constructions" out of sense-data, but I did so only after giving it very serious consideration. I certainly had no intention of ruling it out from the start.

But even if I had ruled out phenomenalism, I think that Mr. Stigen would still be wrong in saying that my conception of sense-datum statements was such as to make it difficult to see how they could lend physical-object statements any support at all. For in fact I so defined them that from the proposition that some physical object was being perceived it logically followed that some sense-datum was being sensed. Now our belief in the existence of physical objects is founded ultimately on our perceiving them; so that if it be granted that our perceiving a physical object essentially involves our sensing some sense-datum, there is surely no difficulty in seeing how physical-object statements can depend upon sense-datum statements for their support. What may have misled Mr. Stigen here is that I do in fact conclude that a physical-object statement does not necessarily entail any sense-datum statement; or, as he puts it, that there need be no truth-relation between them. But the explanation of this is that it is only a special class of physical-object statements which imply that the object is actually perceived. From the point of view of the justification of physical-object statements, this class is all-important, but the actual reference to perception makes it untypical in one vital respect. It would therefore be a mistake to take it as the model for the analysis of all physical-object statements: but it is an even greater mistake to ignore it altogether, as it seems to me that Mr. Stigen does.

I ought perhaps to add here that I did not mean to beg the question against the sceptic by assuming from the outset that physical objects are perceived. I chose to explain the relatively unfamiliar notion of sensing sense-data in terms of the familiar notion of perceiving physical objects, but this was only an expository device. The assumption that physical objects are perceived was not intended to be a logical pre-condition of talking significantly about sense-data. The rule which I tried to lay down was, as I have said, that *if* any physical object is ever perceived, then some sense-datum must be sensed; but *whether* any physical object is ever perceived is a question which I allowed the sceptic freedom to argue. This follows from the fact that,

so far as my introduction of sense-data went, I left it an open question whether, on the evidence of our senses, we are entitled to believe that any physical objects exist.

Since I allowed for there being a logical connection between perceiving physical objects and sensing sense-data, I do not need to take my stand on the weaker assumption, which Mr. Stigen offers to me, that these processes are psychologically connected. Of course I do not deny that there is this psychological connection, but I found no call to lay any weight upon it. For the question which concerned me was not how we come to believe in the existence of physical objects but how this belief can be justified.

I agree with Mr. Stigen that the answer which I eventually give to this question is pretty lame. On the other hand I think that he is wrong in charging me with an *ignoratio elenchi*. He points out, quite truly, that the sceptic does not deny that the passage from sense-data to physical objects is actually made, but only doubts whether it is legitimate; and he argues that I in no way remove this doubt by merely appealing to the linguistic fact that "sentences which are taken as referring to physical objects are used in such a way that our having the appropriate experience counts in favour of their truth". Specifically, his objections are that linguistic usage may be conventional or arbitrary, and therefore cannot provide the answer to a question of "epistemological fact"; that the question at issue is not a question about the use of words but about the relation between two different types of statement; and finally that the sceptic can accept my answer and still preserve his problem by asking why it is "that sentences referring to physical objects are used in such a way that sense-data statements may count in favour of their truth".

I must say that none of these objections seem to me to be valid. It is indeed true that linguistic usage is arbitrary, in the sense that the actual sentences which are understood by us to refer to physical objects might conventionally have been endowed with some other meaning; but given that they have the meaning that they do, we do not need to look further for the source of "epistemological fact." To say that a sentence refers to a physical object is just a way of saying that it has a use of a certain special sort; and the way to determine the sort of use it has is just to consider how one would test the truth or falsity of what it serves to state. I do not know what kind of fact an epistemological fact is supposed to be if it is not based upon the truth-relations of what is expressed by sentences of different types. Neither

does it make any difference here whether we talk of the use of sentences or of statements. For to specify the use of a given sentence is just to give details of the statement which it is understood to express.

There remains the argument that the sceptic can still ask why the one type of statement is evidence for the other. But what would be the force of this question? What answer could there be to it except that this relation is a consequence of their being the sort of statements that they are? In this respect the problem of perception seems different from the problem of other minds or from that of our knowledge of the past, though the difference may be only apparent. It is at least plausible to hold that if statements about the present are to be evidence for statements about the past, some empirical hypothesis must be found to bridge the gap, since it does not appear that the two types of statement are logically connected; and the same may be thought to apply to the case of statements about the overt behaviour of other persons and statements about their "inner" experiences. But then we are faced with the demand that these empirical hypotheses should themselves be justified. In the case of perception, on the other hand, we do have a logical connection on which to build, at least if I am right in thinking that sense-data can be so defined that perceiving a physical object entails sensing a sense-datum. For this makes it possible, in the way I indicated in my book, to treat physical object statements as *interpretations* of sense-datum statements. This still leaves a problem with regard to their analysis, which I claim to have done something to elucidate. I should not say that I had solved it; but I still think that if it were solved there would not be a further problem about their justification.

I admit that it sounds a little strange to say that in referring as we do to physical objects we are elaborating a *theory* with respect to the evidence of our senses. It seems to make physical objects somehow less substantial than we think them to be. I am therefore not surprised that Mr. Stigen wishes to hold that they are real in some more solid sense. He does not make clear, however, what he takes this sense to be. My view allows us to say that physical objects exist, in so far as statements which ostensibly refer to them are very frequently true. Mr. Stigen might argue that this was too weak a criterion of existence in this context, but again I am not at all clear what one could put in its place.

An argument in favour of the view that physical objects, of the common-sense type, are theoretical constructions is that the concep-

tual system in which they figure is not the only one that could be devised to account for our experience. For example, we might employ a system like that which was introduced into physics by the field-theory; one in which regions of space, or units of space-time, were treated as the only individuals; or we might have a system in which the primary particulars were processes or events. I think that any system which could be regarded as a reasonable alternative to the one we have, would be bound to portray some sort of public world; but it does not seem necessary that its methods of individuation should be the same as those which yield us the physical objects of common sense. However, this is a larger question than I can attempt to deal with here.

III. Statements about the Past

With regard to what I had to say about memory images: Mr. Stigen accuses me of misrepresenting the views of Hume and Russell. He claims that in their view the function of the memory image was not to support belief in the past event but to point to the cause of the memory-experience. But this is a false antithesis. If the image did "point to" the past event as the cause of the memory-experience it would support the belief that the event had occurred. And in any case Mr. Stigen has mistaken the point at issue. Whatever role they assigned to the memory image, I was surely right in saying that both Hume and Russell took it to be an essential ingredient of a memory-experience; and I argued that they must be mistaken in this, not only because it was impossible for any image intrinsically to point beyond itself, but also because there could be memory-experiences in which no image played any part at all.

Mr. Stigen also implies that I have been unfair to the naive realist by making him claim infallibility for memory beliefs. But my point was that unless the naive realist holds that some memory beliefs are infallible or, as I preferred to put it, self-guaranteeing, he has no answer to the question how we can know that memory can be trusted. In fact he would have no theory of memory at all beyond maintaining that memory-experiences are unanalyzable. But surely this is not all that was intended by those who have claimed that memory makes us directly acquainted with the past.

I am puzzled too by Mr. Stigen's idea that a reductionist could hold that statements about the past were analyzable into statements about present and future evidence, without holding that they were

dependent on them for their truth. For what could possibly be meant by saying that two statements are equivalent if it does not imply that they coincide in their truth-conditions? Of course it is misleading in such a case to speak of one being evidence of the other, for this suggests that they stand in a weaker relation than that of mutual entailment; but the characteristic feature of reductionism is that it identifies the statement which it seeks to analyze with what would commonly be regarded as the evidence for it. In the case of statements about the past, the available evidence is constantly changing, with the result that, on the reductionist view, the content of these statements, or, to speak more accurately, the meaning of the sentences which express them, must also be in a constant state of change. It was for this reason that I charged the reductionists with making statements about the past "unstable". Mr. Stigen demurs to this, but he appears to have misunderstood me. I was not denying the obvious fact that the acquisition of new evidence may change our *beliefs* about the past.

If I understand him rightly, Mr. Stigen does not dispute my view that sentences in different tenses can express the same statement, so that the question whether it is about the past, present, or future depends not upon the statement as such, but rather on the relation which holds between the time to which it refers and the time at which it is expressed. Mr. Stigen objects only that this "logical" conception of a statement does not help with our epistemological problem about the past. I think that it does help, at least to the extent of enabling us to rebut the reductionist's contention that it is only if they are identified with present or future evidence that statements about the past can be intelligible. Nevertheless I agree with Mr. Stigen that the problem of justification still remains.

Here again I must acknowledge that the conclusion to which I come is very tame. Mr. Stigen finds some difficulty in my statement that "if the fact that one seems to remember an event is a good reason for believing that it occurred, it is only because there is independent evidence that when someone says that he remembers something the chances are that it was so". My reason for making it was that it seemed to me that saying "I remember" was rather a way of staking a claim to knowledge than of giving the justification for it: hence the need for independent evidence. But if this evidence has to be justified in its turn, and so *ad infinitum*, we come to an impasse. It is quite clear to me that our beliefs about the past can be justified only in terms of one another. Accordingly, if the sceptic requires us to give a general

justification of the passage from present experiences to past events, he is asking for what is logically impossible. In this respect the problem of our knowledge of the past is analogous to the problem of induction. I am not entirely happy to leave the matter there, but I still do not see what else is to be done.

IV. Myself and Others

Mr. Stigen says of the final chapter of my book that it is "in many ways the most provocative but also the least clear". No doubt the reason why it seems to him the least clear is that my thoughts on this topic were inchoate, if not incoherent, and I am afraid they still are. I hope, however, that I can succeed in making one or two points a little clearer.

Mr. Stigen says rightly that Hume distinguished the question of justifying our belief in persistent self-identity from that of accounting for its origin; and he accuses me of disregarding this distinction. But the reason why Hume pursued his enquiry into the source of our idea of self-identity was that he thought the idea itself was fictional. He did not see that the relations of "constancy and coherence" on which he believed that the idea was founded could just as well serve as the basis for its analysis. There are, indeed, obvious objections to an analysis of this type which neither Hume nor any of his followers have succeeded in meeting. At the same time it is not at all easy to see what one can put in its place. I am at present inclined to think that personal identity depends upon the identity of the body, and so is governed by the same criteria as those that apply to physical objects in general; but this view also is open to very serious objections, which I do not see fully how to overcome.

On the question of our knowledge of other minds, Mr. Stigen again thinks that I have been unfair to the naive realists. I accused them, here as elsewhere, of dismissing the problem instead of solving it. The naive realist "insists that we do know what we think we know, but he does not explain how it is possible that we should know it". But, says Mr. Stigen, if the naive realist were right in thinking that one can have direct acquaintance with the mental states of others, there would be no problem to solve. To ask me how I knew that my neighbour was in pain would be as inappropriate as to ask me how I knew that I was in pain myself. But while I agree that if this were so the problem would disappear, I do not agree with Mr. Stigen that it is unfair to

expect the naive realist to justify his position. It is certainly not obvious that the question how one knows about another person's state of mind is ever illegitimate. What reason has the naive realist for thinking otherwise? If he can give no reason, then surely his position does warrant my description of it. He insists that he knows such things, but he cannot tell us how.

With regard to the suggestion that the other minds' problem can be solved by drawing a distinction between content and structure, Mr. Stigen says that he cannot find room for it in my classification of the different ways in which one might try to meet the sceptic's attack. But he himself gives the answer to this when he remarks a little later on that the structuralist's thesis is not very different from that of the physicalist. It is, in short, a form of reductionism, since it implies that statements about the experiences of others are to be interpreted as asserting no more than what can be externally observed. On this view, every statement has, as it were, a public and a private face. In its public aspect it refers to a common world, which is entirely a world of structure. This map is then privately coloured by each of us in accordance with the content of our individual experiences. Since this content is incommunicable, the only experiences that one can significantly refer to are one's own. What appear to be references to the experiences of others are in fact statements about a portion of the public world, interpreted in terms of structure, and verified by observations which are private to oneself.

The main objection to this theory that I brought forward in my book was that so far as descriptive statements are concerned the distinction between structure and content is not tenable. Mr. Stigen now offers me the solution that the structural features of these statements are supposed to constitute a purely logical framework; and for confirmation he refers me to the title of Carnap's "*Der logische Aufbau der Welt*". But my answer to this, which I think is borne out by Carnap's book, is that if the structuring is purely logical, it cannot do the work required of it; it cannot yield us a world in which anything happens. Moreover I suspect that when it is generalized this theory is actually inconsistent. For not only do I have to "cash" the public, structural statements in terms of the content of my private experiences; I have to suppose that other people do the same. But I cannot suppose this, the supposition can have no meaning for me, if I am bound to construe any reference to their experiences as a statement about structure. In short, the structuralists cannot have it both

ways. If the statements that I can make about the experiences of others are purely structural, no problem can arise for me about the content of these experiences; but then it also follows that I cannot attribute experiences, in a sense which is not purely structural, to anyone but myself. If I can attribute experiences, in this sense, to others, then I can significantly raise the question of their content; and so the problem of my knowledge of other minds is not avoided.

A similar objection holds against one variety of physicalism; namely that in which statements which refer to the experiences of others are constructed behaviouristically, but statements which refer to one's own experiences are not. For this too is inconsistent, as I showed in my book. Consequently, if the thesis of physicalism is to be maintained at all, it must be taken to apply to all experiences, not only to those of other people but also to one's own. So if it can be shown that it does not apply to one's own case the thesis will have been refuted. Mr. Stigen overlooks this when he argues against me that the consideration of one's own case is irrelevant.

Mr. Stigen's own version of physicalism is that it is a kind of self-denying ordinance. The physicalist decides that he will recognize only those descriptive statements that can be intersubjectively verified; and this means that he does not countenance any reference to anyone's experiences but only to people's physical condition. If this were all, the theory would not be open to objection, except on grounds of policy, but it would also have nothing to contribute to the other minds' problem. In fact, however, the theory is much stronger than this, as Mr. Stigen himself implies when he correctly attributes to Carnap the view that the several protocol languages, though unconnected with one another, are all translatable into the one physical language. For if this is so, it is not just a matter of one's choosing not to refer to experiences or to mental states, as distinct from their physical manifestations; it is that one could not do so if one tried. Unless a statement can be construed as referring to a physical event, it is not intelligible.

In view of this, it is inconsistent of Carnap to talk of the experiences of various persons as belonging to "completely separated and mutually disconnected realms". For he is committed to holding that, in the only sense in which we can significantly talk of experiences at all, they all belong to the same realm, the one public physical world. Like the structuralists, he wants to have it both ways; both to concede to commonsense that we all do have private experiences, and to dissolve

the problem of our knowledge of other minds by maintaining that the only assertions that we can significantly make about other people refer to what is publicly observable. But it is clear that these two standpoints cannot be reconciled.

I am aware that merely to exclude the naive realist and physicalist solutions is not to solve the other minds' problem. Mr. Stigen objects to my trying to show that statements about the experiences of others should be regarded as verifiable, on the ground that even if this is true it is irrelevant. But the answer to this is that unless I can show that these statements are verifiable, I am exposed to the argument that they are not intelligible; and unless I can show that they are at least intelligible, the argument from analogy, on a form of which I finally rely, cannot even get under weigh. I am not at all happy with this argument from analogy, but I still prefer it to anything that has been offered in its place.

V. Verification and Justification

In conclusion, I should like to comment briefly on Mr. Stigen's remarks concerning verification and justification, which he thinks that I do not sufficiently distinguish. I think that he is wrong in saying that it is possible to believe a true statement without knowing what would constitute its verification. In my view, to know what would constitute the verification of a statement is a condition of understanding it; and presumably when Mr. Stigen talks of our believing true statements he has in mind statements which we understand. But perhaps what he meant to say was that we can believe a true statement without knowing whether it can actually be verified, and with this of course I should agree.

I am not sure either that Mr. Stigen is right in saying that successful verification is a pre-condition of knowledge. For I think that we can reasonably claim to have some knowledge of the future, even if it is mainly negative in character, and clearly no statement about the future can already have been verified. It may be, indeed, that Mr. Stigen means to assert no more than that it is a pre-condition of one's knowing any statement to be true that one should already have acquired some evidence in its favour; but I think that even this is questionable, as I tried to show in the first section of my reply. In the light of what I said there, the most that I can concede to Mr. Stigen is that it is a pre-condition of one's knowing a statement to be

true either that one should have evidence in favour of it or that there should be evidence of one's having known statements of that kind.

I agree with him, however, on the point to which he attaches most importance, namely that more is required for the justification of a claim to knowledge than that the statement in question should be capable of being verified. The situation of the person who makes the claim has also to be taken into account. It follows, as he says, that one person may know a proposition which another only believes. He is wrong, however, in saying that I ignored this point or that I made no effort to explain it. I thought that it came out quite clearly in the account that I gave of knowledge.

Mr. Stigen ends by saying that we need some bases, like Russell's "momentary premises" or C. I. Lewis's "terminating judgements", as bulwarks against the sceptic's attack. I agree that we need such bases, though I doubt if they can ever have the complete security which has sometimes been attributed to them. But this is not all that we need. As I tried to show in my book, the sceptic's main attack is directed not so much against the bases from which we start as against the ways in which we try to build upon them. To examine these procedures, and if possible to vindicate them, is the most important task of the theory of knowledge. I believe that I have gone a little way towards its completion; but I fully agree with Mr. Stigen that there is a great deal more that still remains to be done.

ABSTRACTS

From "Philosophy of Science"

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The Einstein-Podolsky-Rosen paradox re-examined, DAVID H. SHARP. This paper discusses the Einstein-Podolsky-Rosen paradox from a new point of view. In section II, the arguments by which Einstein, Podolsky and Rosen reach their paradoxical conclusions are presented. They are found to rest on two critical assumptions: (a) that *before* a measurement is made on a system consisting of two non-interacting but correlated sub-systems, the state of the entire system is exactly represented by:

$$\psi_a(\bar{r}_1, \bar{r}_2) = \sum_{\eta} a_{\eta} \tau_{\eta}(r_1, r_2) = \sum_{i,k} a_{ik} \psi_i(\bar{r}_1) \sigma_k(\bar{r}_2)$$

(b) that the *exact* measurement of an observable A in one of the sub-systems is possible.

In section III it is shown that assumption (b) is incorrect. Thus we conclude, as did Bohr, that the results of Einstein, Podolsky and Rosen are not valid.

The arguments of section III are quite distinct from Bohr's, and therefore in Section IV this work is related to that of Bohr.

Comments on the paper of David Sharp, HILARY PUTNAM.

The scientific philosophy, JAMES K. FEIBLEMAN.

Kinds and criteria of scientific laws, MARIO BUNGE. Factual statements that might qualify for the status of law statements are classed from various philosophically relevant standpoints (referents, precision, structure of predicates, extension, systematicity, inferential power, inception, ostensiveness, testability, levels, and determination categories). More than seven dozen of not mutually exclusive kinds of lawlike statements emerge. Strictly universal and counterfactually powerful statements are seen to constitute just one kind of lawlike statements; classificatory and some statistical laws, e.g., are shown not to comply with the requirements of universality and counterfactual force.

Conditions for lawlike statements to be called laws are then examined, and a liberal criterion of lawfulness is finally proposed, which reads thus: A proposition is a law statement if and only if it is *a posteriori* (not logically true), general in some respect (does not refer to unique objects), has been satisfactorily corroborated for the time being in some domain, and belongs to a theory (whether adult or embryonic). It is claimed that criteria of laws change alongside with the emergence of new usages of the term 'law', and that by adopting a liberal criterion of lawfulness we would conform to contemporary usage and would cease inhibiting the search for regularities in the sciences of man.

The natural system in biology, J. LORCH. Prior to the advent of evolutionary theory the Natural System was generally conceived as based on "distinctions of kind, not consisting in a given number of definite properties" (J. S. Mill). It was considered final and unique, to be arrived at by more than one approach. Evolutionary theory has shifted emphasis to different characters, yet explicitly or implicitly the belief in a final natural system in biology persists in many textbooks and taints research. Allegedly natural systems are shown to be fundamentally arbitrary and artificial, dependent on numerous value judgments and intrinsically conflicting to varying degrees. Because of this and because it has turned into a bar to clear thinking, the adjective "natural" should be abolished.

Physical and social kinship, J. A. BARNES.

Explanations and their justifications, RUDOLPH H. WEINGARTNER.

Comments on Weingartner, MICHAEL SCRIVEN.

Vol. 28, No. 4, October 1961

Ontological induction and the logical Typology of scientific variables, WILLIAM W. ROZEBOOM. It is widely agreed among philosophers of science today that no formal pattern can possibly be found in the origins of scientific theory. There is no such thing as a "logic of discovery," insists this view — a scientific hypothesis is susceptible to methodological critique only in its relation to empirical consequences derived *after* the hypothesis itself has emerged through a spontaneous creative inspiration. Yet confronted with the tautly directed thrust of theory-building as actually practiced at the cutting edge of scientific research, this romantic denial of method in the genesis of ideas takes on the appearance of myth.

It is the contention of this article that as empirical data ramify in logical complexity, they deposit a hard sediment of theory according to a standard inductive pattern so primitively compelling that it must be recognized as one of the primary forms of inferential thought. This process, here called "ontological induction," is a distillation out of unwieldily observed regularities of more conceptually tractable states hypothesized to underlie them, and is the wellspring of our beliefs in theoretical entities. Previous failure to recognize this pattern of induction has undoubtedly been in substantial measure a result of inadequate attention to the structural details of scientific propositions; for in order to exhibit the nature of ontological induction clearly, it is first necessary to make extended forays through sparsely explored methodological terrain — notably, the nature of scientific "variables," the logical form of "laws", and the typehierarchy of scientific concepts.

Probability concepts in Quantum mechanics, PATRICK SUPPES. The fundamental problem considered is that of the existence of a joint probability distribution for momentum and position at a given instant. The philosophical interest of this problem is that for the potential energy functions (or Hamiltonians) corresponding to many simple experimental situations, the joint "distribution" derived by the methods of Wigner and Moyal is not a genuine probability distribution at all. The

implications of these results for the interpretation of the Heisenberg uncertainty principle are analyzed. The final section consists of some observations concerning the axiomatic foundations of quantum mechanics.

Definition and reduction, EDWARD H. MADDEN. While I do not accept any current analysis of theoretical terms I also reject certain criticisms of them. Specifically, I reject the criticism that the paradoxes of material implication and the counterfactual problem eliminate the explicit definition view; and I also reject the criticism that explicitly defined theoretical terms do not refer to anything which "really exists" or do not have "excess meaning." I do argue, however, that the explicit definition view confuses and conflates the concepts of criterion and meaning analysis. I also defend reduction sentences against the counterfactual difficulty, but show, too, how this view is already logically committed to the network or postulational view of meaning. Finally, I show how the concept of reduction sentences confuses in several ways concepts of criterion and meaning analysis — although not in quite the same way as explicit definitions do.

The concept of information and the unity of science, JOHN WILKINSON. An attempt is made in this paper to analyze the purely formal nature of information-theoretic concepts. The suggestion follows that such concepts, used to supplement the logical and mathematical structure of the language of science, represent an addition to this language of such a sort as to allow the use of a unitary language for the description of phenomena. (The alternative to this approach must be certain multi-linguistic and mutually untranslatable descriptions of related phenomena, as with the various versions of Complementarity).

This conception is tested for the specific case of Heisenberg's Uncertainty Principle, in order to show that, with the assumption of a suitable and intuitively satisfactory definition of the quantity of information contained in a measurement, the Heisenberg Principle becomes an informational restriction arising from the formal properties of the symbols of a given language rather than as a "law" of nature.

Probabilities as Truth-value estimates, HUGUES LEBLANC. The author recently claimed that $\text{Pr}(P, Q)$, where Pr is a probability function and P and Q are two sentences of a formalized language L , qualifies as an estimate—made in the light of Q —of the truth-value of P in L . To substantiate his claim, the author establishes here that the two strategies lying at the opposite extremes of the spectrum of truth-value estimating strategies meet the first five of the six requirements (R1-R6) currently placed upon probability functions and fail to meet the sixth one. He concludes from those two results that the value for P and Q of any function satisfying R1-R5 must rate "minimally satisfactory" and the value for P and Q of any function satisfying R1-R6 must rate "satisfactory" as an estimate—made in the light of Q —of the truth-value of P in L .

Hempel and Oppenheim on explanation, ROLF EBERLE, DAVID KAPLAN, and RICHARD MONTAGUE. Hempel and Oppenheim, in their paper 'The Logic of Explanation', have offered an analysis of the notion of scientific explanation. The present paper advances considerations in the light of which their analysis seems inadequate. In

particular, several theorems are proved with roughly the following content: between almost any theory and almost any singular sentence, certain relations of explainability hold.

Explanation revisited, DAVID KAPLAN. In 'Hempel and Oppenheim on Explanation', (see preceding article) Eberle, Kaplan, and Montague criticize the analysis of explanation offered by Hempel and Oppenheim in their 'Studies in the Logic of Explanation'. These criticisms are shown to be related to the fact that Hempel and Oppenheim's analysis fails to satisfy simultaneously three newly proposed criteria of adequacy for any analysis of explanation. A new analysis is proposed which satisfies these criteria and thus is immune to the criticisms brought against the earlier analysis.

Semantic paradoxes and the propositional analysis of indirect discourse, NICHOLAS RESCHER.

